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EDITED BY
T. A. A. BROADBENT, M.A

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VOL. XV. MARCH, 1931. No. 212.

The Mathematical Association.

THE Annual Meeting of the Mathematical Association was held at the London Day Training College on 5th-6th January, 1931.

On Monday, 5th January, the proceedings opened at 2.30 p.m. with the transaction of formal business, the President, Sir Arthur Eddington, being in the chair.

The Report of the Council * for 1930 was considered and adopted. The Treasurer's statement, to be followed by a full, audited statement of accounts to be published in the *Gazette*, was accepted.

The election of Officers and Council for 1931 was made, including the addition of Dr. F. S. Macaulay's name to the list of Vice-Presidents, and the substitution of Miss D. M. Clark and Mr. A. S. Gosset-Tanner for Miss D. R. Smith and Prof. Piaggio as members of the Council.

The proposed alterations in the rules as set out in the agenda were passed.

Sir Arthur Eddington then delivered his Presidential Address on *The End of the World (from the Standpoint of Mathematical Physics)*.† This was followed, after an interval, by Prof. A. R. Forsyth's lecture, *Dimensions in Geometry*.‡

On Tuesday, 6th January, at 10 a.m., Mr. A. Robson opened a discussion on *The Report on the Teaching of Mechanics*.§ After an interval, a discussion on *Gambling* || was opened by papers from Mr. W. Hope-Jones, Dr. F. J. W. Whipple and Mr. P. M. Marples.

The afternoon sitting opened at 2.30 p.m. with Prof. J. E. A. Steggall's lecture entitled *Faith and Reason in beginning the Calculus and elsewhere*, followed after tea by a lecture from Prof. E. H. Neville, *Limits in Geometry*.¶

The usual votes of thanks brought the proceedings to a close.

A Publishers' Exhibition was arranged for the two days, and was open till 7 p.m. on Tuesday.

* Pp. 314-315. † Pp. 316-324. ‡ Pp. 325-338. § Pp. 339-346.
|| Pp. 347-358. ¶ Will be published later.

REPORT OF THE COUNCIL FOR THE YEAR 1930.

During the year 1930, 119 new members of the Association have been admitted. The number on the Roll is now 1275: of these 5 are Honorary Members, 68 are Life Members by composition, 16 are Life Members under the old rule, and 1186 are ordinary members.

The Council regrets to have to record the deaths of Mr. S. H. Anderton, Principal of Alpha Preparatory School, Harrow; Mr. O. I. Burgess, of Marlborough College; Professor Emeritus Florian Cajori, Ph.D., of the University of California; Major C. H. Chepmell, R.A.; Miss A. B. Collier, formerly Vice-Principal and Tutor of Newnham College, Cambridge; Mr. W. J. Greenstreet, Editor of *The Mathematical Gazette*; and Professor H. H. Turner, D.Sc., F.R.S., who was President of the Association for the years 1909 and 1910.

The Council desires to place on record its deep sense of the loss sustained by the Association in the death of Mr. Greenstreet and its very warm appreciation of his devoted work for so many years as Editor of *The Mathematical Gazette*, in which capacity he rendered invaluable service not only to the Association but also to a much wider field of teachers and scholars.

The warmest thanks of the Association are due to Professor E. H. Neville, who took up the Editorship of the *Gazette* when, in June, it fell from the hands of Mr. Greenstreet, and so made it possible for the issue of the *Gazette* to be continued without a break. This is the second time that Professor Neville has helped the Association in a serious emergency.

To fill the vacancy caused by the death of Mr. Greenstreet, the Council has appointed Mr. T. A. A. Broadbent, of the University of Reading, to the office of Editor of the *Gazette*. Mr. Broadbent will take up his duties at the beginning of the new year.

The Council nominates Dr. F. S. Macaulay, F.R.S., to be a Vice-President of the Association, as a mark of appreciation of his past services in connection with the *Gazette*.

During 1930 a Report on "The Teaching of Mechanics in Schools" has been published and a copy of it has been sent to each member of the Association.

Recommendations have been sent by the General Teaching Committee to the authorities of London University on the subjects of "Trigonometry in School Certificates" and "the Group D Papers of the London Higher Certificate". These recommendations have been favourably received. Similar recommendations are being sent to other Examining bodies.

The General Teaching Committee has also instituted an inquiry into the attitude of the Public Schools to the "Report on Entrance Scholarships from Preparatory Schools", and the results are being considered by the Committee.

The Boys' Schools Committee is engaged in preparing reports on the teaching of elementary mathematics, including Trigonometry. The school period to be covered is roughly from Preparatory School Stage to School Certificate Stage, and it is hoped the reports will be of value to teachers in schools of all types: to this end the Committee has co-opted members from Preparatory and Central Schools. To make its reports accessible as soon as possible each subject is being considered in turn: the Arithmetic report is well on the way to completion and should be issued in 1931. The Committee welcomes contributions of memoranda from members of the Association. They should be sent to the Secretary of the Committee, Mr. C. T. Daltry, 112, Canberra Road, London, S.E. 7.

The Girls' Schools Committee is conducting an inquiry into the character of the Entrance Examinations in Mathematics set for pupils passing from Elementary to Secondary Schools.

The Council has decided to issue a new edition of the rules of the Association, incorporating changes which have been made since January 1926, and a few additional changes which are being submitted for the consideration of the Annual Meeting.

The year has seen a further considerable increase in the value of the Library and in the use made of it. A Third List of Books and Pamphlets, issued in July, gave details of the additions from 1926 to 1929. This list combines with its two predecessors to give a complete record of the contents of the Library, other than periodicals, at the end of 1929—a record which occupies 150 pages. The binding of pamphlets and off-prints has followed their cataloguing, and the Library has now 64 volumes of these, each indexed from the proper list. Mr. F. Beames has continued to give great help to the Librarian.

By the death of Mr. Greenstreet the Library suffered a great loss: his benefactions were incessant, his immense bibliographical knowledge was always at the service of inquirers, and there was not a book of his own which he was not ready to lend. Under his will the Association comes sadly into possession of several runs of periodicals and of about 300 volumes besides.

The Council desires to express its thanks to Miss D. R. Smith and to Professor H. T. H. Piaggio, who now retire from the Council, for the services which they have rendered to the Association during their tenure of office as members of the Council since 1927 and 1926 respectively.

GLEANINGS FAR AND NEAR.

799. I set skilful workmen to make me Quadrants, Staves, Semi-circles, etc., as much, namely, as concern the Fabrick of them; not trusting to their mechanick hands to divide them; but had them divided by an ingenious Practitioner in the Mathematicks.—*The Dangerous Voyage of Capt. Thomas James*, 1631.

THE END OF THE WORLD.

(FROM THE STANDPOINT OF MATHEMATICAL PHYSICS.)

By Prof. Sir Arthur S. Eddington, D.Sc., F.R.S.

I am afraid that the title of my address is not quite explicit. It lacks that precision of statement which ought to characterise the utterances of a mathematician. I have undertaken to speak to you about the End of the World, but I have not told you *which end*. The world—or space-time—is a four-dimensional continuum, and consequently offers a choice of a great many directions in which we might start off to look for an end; and it is by no means easy to describe "from the standpoint of mathematical physics" the direction in which I intend to lead you. I have therefore to examine at some length the preliminary question, Which end? But that leads us so deeply into the whole problem that by the time I have finished with it my address will be over.

We no longer look for an end to the world in its space dimensions. We have reason to believe that so far as its space dimensions are concerned the world is of spherical type. If we proceed in any direction in space we do not come to an end of space, nor do we continue on to infinity; but, after travelling a certain distance (not inconceivably great), we find ourselves back at our starting point, having "gone round the world". A continuum with this property is said to be finite but unbounded. The surface of a sphere is an example of a finite but unbounded two-dimensional continuum; our actual three-dimensional space is believed to have the same kind of connectivity, but naturally the extra dimension makes it more difficult to picture. If we attempt to picture spherical space we have to keep in mind that it is the *surface* of the sphere that is the analogue of our three-dimensional space; the inside and the outside of the sphere are fictitious elements in the picture which have no analogue in the actual world.

We have recently learnt, mainly through the work of Prof. Lemaître, that this spherical space is expanding rather rapidly. In fact if we wish to travel round the world and get back to our starting point, we shall have to move faster than light; because whilst we are loitering on the way the track ahead of us is lengthening. It is like trying to run a race in which the finishing-tape is moving ahead faster than the runners. We must therefore picture the stars and galaxies as embedded in the surface of a rubber balloon which is being steadily inflated; so that, apart from their individual motions and the effects of their ordinary gravitational attraction on one another, celestial objects are becoming further and further apart simply by the inflation. It is probable that the spiral nebulae are so distant that they are very little affected by mutual gravitation and exhibit the inflation effect in its pure form. It has been known for some years that they are scattering apart rather rapidly, and we accept their measured rate of recession as a determination of the rate of expansion of the world.

From the astronomical data it appears that the original radius of space was 1200 million light years. Remembering that distances of celestial objects up to several million light years have actually been measured, that does not seem overwhelmingly great. At that radius the mutual attraction of the matter in the world was just sufficient to hold it together and check the tendency to expand. But this equilibrium was unstable. An expansion began, slow at first, but the more widely the matter was scattered the less able was the mutual gravitation to check the expansion. We do not know the radius of space to-day, but I should estimate that it is not less than 10 times the original radius.

At present our numerical results depend on astronomical observations of the speed of scattering apart of the spiral nebulae. But I believe that theory is

well on the way to obtaining the same results independently of astronomical observation. Out of the recession of the spiral nebulae we can determine not only the original radius of the universe, but also the total mass of the universe and hence the total number of protons in the world. I find this number to be either 7×10^{78} or 14×10^{78}.* I believe that this number is very closely connected with the ratio of the electrostatic and the gravitational units of force, and, apart from a numerical coefficient, is equal to the square of the ratio. If F is the ratio of the electrical attraction between a proton and electron to their gravitational attraction, we find $F^2 = 5{\cdot}3 \,.\, 10^{78}$. There are theoretical reasons to believe that the total number of particles in the world is αF^2, where α is a simple geometrical factor (perhaps involving π). It ought to be possible before long to find a theoretical value of α, and so make a complete connection between the observed rate of expansion of the universe and the ratio of electrical and gravitational forces.

I must not dally over space any longer but must turn to time. The world is closed in its space dimensions but is open in both directions in its time dimension. Proceeding from "Here" in any direction in space we ultimately come back to "Here"; but proceeding from "Now" towards the future or the past we shall never come across "Now" again. There is no bending round of time to bring us back to the moment we started from. In mathematics this difference is provided for by the symbol $\sqrt{-1}$, just as the same symbol crops up in distinguishing a closed ellipse and an open hyperbola.

If, then, we are looking for an end of the world—or, instead of an end, an indefinite continuation for ever and ever—we must start off in one of the two time directions. How shall we decide which of these two directions to take? It is an important question. Imagine yourself planked down in some unfamiliar part of space-time so as not to be biassed by conventional landmarks or traditional standards of reference. There ought to be a sign-post with one arm marked "To the Future" and the other arm marked "To the Past". My first business is to find this sign-post, for if I make a mistake and go the wrong way I shall lead you to what is no doubt an "end of the world", but it is that end which is more usually described as the *beginning*.

In ordinary life the sign-post is provided by consciousness. Or perhaps it would be truer to say that consciousness does not bother about sign-posts; but wherever it is planted down it buzzes off on urgent business in a particular direction, and the physicist meekly accepts its lead and labels the course it takes "To the Future". It is an important question whether consciousness in selecting its direction is guided by anything in the physical world. If it is guided, we ought to be able to find directly what it is in the physical world which makes it a one-way street for conscious beings? The view is sometimes held that the "going on of time" does not exist in the physical world at all and is a purely subjective impression. According to that view the difference between past and future in the material universe has no more significance than the difference between right and left. The fact that experience presents space-time as a cinematograph film which is always unrolled in a particular direction is not a property or peculiarity of the film (*i.e.* the physical world) but of the way it is inserted into the cinematograph (*i.e.* consciousness). In fact the one-way traffic in time arises from the way our consciousness is geared on to our material bodies:

> Nature has made our gears in such a way
> That we can never get into reverse.

If this view is right, "the going on of time" should be dropped out of our picture of the physical universe. Just as we have dropped the old geocentric outlook and other idiosyncrasies of our circumstances as observers, so we must

* This ambiguity is inseparable from the operation of counting the number of particles in finite but unbounded space. It is impossible to tell whether the protons have been counted once or twice over.

drop the dynamic presentation of events which is no part of the universe itself but is introduced in our peculiar mode of apprehending it. In particular we must be careful not to treat a past-to-future presentation of events as truer or more significant than a future-to-past presentation. We must, of course, drop the theory of evolution, or at least set alongside it a theory of anti-evolution as equally significant.

If anyone here holds this view, I have no argument to bring against him. I can only adopt the common resource when arguments fail—I proceed to cast aspersions on his character. I say to him, "You are a teacher whose duty it is to inculcate in youthful minds a true and balanced outlook. But you teach (or without protest allow your colleagues to teach) the utterly one-sided doctrine of evolution. You teach it not as a colourless schedule of facts but as though there were something significant, perhaps even morally inspiring, in the progress from formless chaos to perfected adaptation. This is dishonest; you should also treat it from the equally significant point of view of anti-evolution and discourse on the progress from future to past. Show how from the diverse forms of life existing to-day Nature anti-evolved forms which were more and more unfitted to survive until she reached the sublime crudity of the paleozoic forms. Show how from the solar system Nature anti-evolved a chaotic nebula. Show how in the course of progress from future to past Nature took a universe which with all its faults is not such a bad effort of architecture and—in short, made a hash of it.

Leaving aside the guidance of consciousness, we have found it possible to discover a kind of sign-post for time in the physical world. The sign-post is of rather a curious character, and I would hardly venture to say that the discovery of the sign-post amounts to the same thing as the discovery of an objective "going on of time" in the universe. But at any rate it serves to discriminate past and future, whereas there is no corresponding objective distinction of left and right. The distinction is provided by a certain measurable quantity called entropy. Take an isolated system and measure its entropy S at two instants t_1 and t_2. We want to know whether t_1 is earlier or later than t_2 without employing the intuition of consciousness, which is too disreputable a witness to trust in mathematical physics. The rule is that the instant which corresponds to the greater entropy is the later. In mathematical form

$$dS/dt \text{ is always positive.}$$

This is the famous second law of thermodynamics.

Entropy is a very peculiar conception quite unlike the conceptions ordinarily employed in the classical scheme of physics. We may most conveniently describe it as the measure of disorganisation of a system. Accordingly our sign-post for time resolves itself into the law that disorganisation increases from past to future. It is one of the most curious features of the development of physics that the entropy outlook grew up quietly alongside the ordinary analytical outlook for a great many years. Until recently it always played second fiddle; it was convenient for getting practical results, but it did not pretend to convey the most penetrating insight. But now it is making a bid for supremacy, and I think there is little doubt that it will ultimately drive out its rival.

There are some important points to emphasise. First there is no other independent sign-post for time; so that if we discredit or "explain away" this property of entropy, the distinction of past and future in the physical world will disappear altogether. Second, the test works consistently; isolated systems in different parts of the universe agree in giving the same direction of time. Third, in applying the test we must make certain that our system is strictly isolated. Evolution teaches us that more and more highly organised systems develope as time goes on; but this does not contradict the conclusion that on the whole there is a loss of organisation. It is partly a question of

definition of organisation; from the evolutionary point of view it is quality rather than quantity of organisation that is noticed. But, in any case, the high organisation of these systems is obtained by draining organisation from other systems with which they come in contact. A human being as he grows from past to future becomes more and more highly organised—at least he fondly imagines so. But if we make an isolated system of him, that is to say if we cut off his supply of food and drink and air he speedily attains a state which everyone would recognise as "a state of disorganisation".

It is possible for the disorganisation of a system to become complete. The state then reached is called thermodynamic equilibrium. The entropy can increase no further, and, since the second law of thermodynamics forbids a decrease, it remains constant. Our sign-post for time disappears; and so far as that system is concerned time ceases to go on. That does not mean that time ceases to exist; it exists and extends just as space exists and extends, but there is no longer any one-way property. It is like a one-way street on which there is never any traffic.

Let us return to our sign-post. Ahead there is ever-increasing disorganisation. We notice one strange fact, that although the sum total of organisation is diminishing certain parts of the universe are exhibiting a more and more highly specialised organisation. That is the phenomenon of evolution. But ultimately this must be swallowed up in the advancing tide of chance and chaos, and the whole universe will reach a state of complete disorganisation—a uniform featureless mass in thermodynamic equilibrium. This is the End of the World. Time will *extend* on and on presumably to infinity. But there will be no definable sense in which it can be said to *go* on. Consciousness will obviously have disappeared from the physical world before thermodynamical equilibrium is reached, and dS/dt having vanished, there will remain nothing to point out a direction in time.

It is more interesting to look in the opposite direction—towards the past. Following time backwards we find more and more organisation in the world. If we are not stopped earlier we must come to a time when the matter and energy of the world had the maximum possible organisation. To go back further is impossible. We have come to an abrupt end of space-time—only we generally call it the "beginning".

I have no philosophical axe to grind in this discussion. Philosophically the notion of a beginning of the present order of Nature is repugnant to me. I am simply stating the dilemma to which our present fundamental conception of physical law leads us. I see no way round it; but whether future developments of science will find an escape I cannot predict. The dilemma is this.—Surveying our surroundings we find them to be far from a "fortuitous concourse of atoms". The picture of the world, as drawn in existing physical theories, shows arrangement of the individual elements for which the odds are multillions * to one against an origin by chance. Some people would like to call this non-random feature of the world purpose or design. I hardly go so far as that; I will call it non-committally anti-chance. We are unwilling to admit in physics that anti-chance plays any part in the reactions between the systems of billions of atoms and quanta that we study; and indeed all our experimental evidence goes to show that these are governed by the laws of chance. Accordingly we sweep anti-chance out of the laws of physics—out of the differential equations. Naturally, therefore, it reappears in the boundary conditions, for it must be got into the scheme somewhere. By sweeping it far enough away from the sphere of our current physical problems, we fancy we have got rid of it. It is only when some of us are so misguided as to try to get back billions of years into the past that we find the sweepings all piled up like a high wall and forming a boundary—a beginning of time—which we cannot climb over.

* I use "multillions" as a general term for numbers of order $10^{10^{10}}$ or larger.

A way out of the dilemma has been proposed which seems to have found favour with a number of scientists. I oppose it because I think it is untenable. But I would like you to understand that I do not oppose it out of any desire to retain the present dilemma. I should like to find a genuine loophole. But that does not alter my conviction that the loophole that is at present being advocated is a blind alley. I must first deal with a minor criticism.

I have sometimes been taken to task for not sufficiently emphasising in my discussion of these problems that the results about entropy are a matter of probability, not of certainty. I said just now that if we observe a system at two instants the instant corresponding to the greater entropy will be the later. Strictly speaking I ought to have said that for a smallish system the chances are, say, 10^{20} to 1, that it is the later. Was it very wrong of me not to mention the 1 in 10^{20} chance that it might be the earlier? Some critics seem to have been shocked at my lax morality in making such a statement, when I was well aware of the 1 in 10^{20} chance of its being wrong. Let me make a confession. I have in the past 25 years written a good many papers and books, broadcasting a large number of statements about the physical world. I fear that not many of these statements run so small a risk of error—and I might add that not many of my candid critics express their doubt in my infallibility so mildly. Except in the domain of pure mathematics the trustworthiness of my conclusions is usually to be rated at nearer 10 to 1 than 10^{20} to 1; even that may be too boastful. I do not think it would be for the benefit of the world that no statement should be allowed to be made if there were a 1 in 10^{20} chance of its being untrue. Conversation would languish somewhat. The only persons entitled to open their mouths would presumably be the pure mathematicians.

The loophole to which I referred depends on the occurrence of chance fluctuations. If we have a number of particles moving about at random they will in the course of time go through every possible configuration, so that even the most orderly, the most non-chance, configuration will occur by chance if only we wait long enough. When the world has reached complete disorganisation (thermodynamic equilibrium) there is still infinite time ahead of it, and its elements will thus have opportunity to take up every possible configuration again and again. If we wait long enough a number of atoms will, just by chance, arrange themselves in systems as they are at present arranged in this room; and, just by chance, the same sound waves will come from one of these systems of atoms as are at present emerging from my lips; they will strike the ears of other systems of atoms, arranged just by chance to resemble you, and in the same stages of attention or somnolence. This mock Mathematical Association meeting must be repeated many times over—an infinite number of times in fact—before t reaches $+\infty$. Don't ask me whether I expect you to believe that this will really happen *:

"Logic is logic. That's all I say."

So after the world has reached thermodynamical equilibrium the entropy remains steady at its maximum value, except that once in a blue moon the absurdly small chance comes off and the entropy drops appreciably below its maximum value. When this fluctuation has died out there will again be a very long wait for another coincidence giving another fluctuation. It will take multillions of years, but we have all infinity of time before us. There is no limit to the amount of the fluctuation, and if we wait long enough we shall come across a big fluctuation which will take the world as far from thermodynamical equilibrium as it is at the present moment. If we wait for an enormously longer time, during which this huge fluctuation is repeated untold numbers of times, there will occur a still larger fluctuation which will take the world as far from thermodynamic equilibrium as it was one second ago.

* I am hopeful that the doctrine of the "expanding universe" will intervene to prevent its happening.

The suggestion is that we are now on the downward slope of one of these fluctuations. It has quite a pleasant subtlety. Is it chance that we happen to be running down the slope and not toiling up the slope? Not at all. So far as the physical universe is concerned we have *defined* the direction of time as the direction from greater to less organisation, so that on whichever side of the mountain we stand our sign-post will point downhill. In fact, on this theory, the going on of time is not a property of time in general, but is a property of the slope of the fluctuation on which we are standing. Again, although the theory postulates a universe involving an extremely improbable coincidence, it provides an infinite time during which the most improbable coincidence might occur. Nevertheless I feel sure that the argument is fallacious.

If we put a kettle of water on the fire there is a chance that the water will freeze. If mankind goes on putting kettles on the fire until $t=\infty$, the chance will one day come off and the individual concerned will be somewhat surprised to find a lump of ice in his kettle. But it will not happen to *me*. Even if tomorrow the phenomenon occurs before my eyes I shall not explain it this way; I shall probably say the devil is in it. I would much sooner believe in the devil than in a coincidence of that kind coming off. And in doing so I shall be acting as a rational scientist. The reason why I do not at present believe that devils interfere with my cooking arrangements and other business, is because by observation I have become convinced by experience that Nature obeys certain uniformities which we call laws. I am convinced because these laws have been tested over and over again. But it is possible that every single observation from the beginning of science, which has been used as a test, has just happened to fit in with the law by a chance coincidence. It is an improbable coincidence, but I think not quite so improbable as the coincidence involved in my kettle of water freezing. So if the event happens and I can think of no other explanation, I shall have to choose between two highly improbable coincidences: (*a*) that there are no laws of Nature and that the apparent uniformities so far observed are merely coincidences, (*b*) that the event is entirely in accordance with the accepted laws of Nature, but that an improbable coincidence has happened. I choose the former because mathematical calculation indicates that it is the less improbable. You see that I reckon a sufficiently improbable coincidence as something much more disastrous than a violation of the laws of Nature; because my whole reason for accepting the laws of Nature rests on the assumption that improbable coincidences do not happen—at least that they do not happen in my experience.*

And similarly if logic predicts that a mock meeting of the Mathematical Association will occur just by a fortuitous arrangement of atoms before $t=\infty$, I reply that I cannot possibly accept that as being the explanation of a meeting of the Mathematical Association in $t=1931$. We must be a little careful over this, because there is a trap for the unwary. The year 1931 is not an absolutely random date between $t=-\infty$ and $t=+\infty$. We must not argue that because for only $1/x$th of time between $t=\infty$ and $t=-\infty$ a fluctuation as great as the present one is in operation, therefore the chances are x to 1 against such a fluctuation occurring in the year 1931. For the purposes of the present discussion the important characteristic of the year 1931 is that it belongs to a period during which there exist in the universe beings capable of speculating about the universe and its fluctuations. Now I think it is clear that such creatures could not exist in a universe in thermodynamical equilibrium. A considerable degree of deviation is required to permit of living beings. Therefore it is perfectly fair for supporters of this suggestion to wipe out of account all those multillions of years during which the fluctuations are less than the minimum required to permit of the development and existence

* No doubt "extremely improbable" coincidences occur to all of us, but the improbability is of an utterly different order of magnitude from that concerned in the present discussion.

of mathematical physicists. That greatly diminishes x, but the odds are still overpowering. The *crude* assertion would be that (unless we admit something which is not chance in the architecture of the universe), it is practically certain that at any assigned date the universe will be almost in the state of maximum disorganisation. The *amended* assertion is that (unless we admit something which is not chance in the architecture of the universe), it is practically certain that a universe containing mathematical physicists will at any assigned date be in the state of maximum disorganisation which is not inconsistent with the existence of such creatures. I think it is quite clear that neither the original nor the amended version applies. We are thus driven to admit anti-chance; and apparently the best thing we can do with it is to sweep it up into a heap at the beginning of time, as I have already described.

The connection between our entropy sign-post and that dynamic quality of time which we describe as "going on" or "becoming" leads to very difficult questions which I cannot discuss here. The puzzle is that the sign-post seems so utterly different from the thing of which it is supposed to be the sign. The one thing on which I have to insist is that, apart from consciousness, the increase of entropy is the only trace that we can find of a one-way direction of time. I was once asked a ribald question: How does an electron (which has not the resource of consciousness) remember which way time is going? Why should it not inadvertently turn round and, so to speak, face time the other way? Does it have to calculate which way entropy is increasing in order to keep itself straight? I am inclined to think that an electron does do something of that sort. For an electric charge to face the opposite way in time is the same thing as to change the sign of the charge. So if an electron mistook the way time was going it would turn into a positive charge. Now, it has been one of the troubles of my friend Dr. Dirac that in the mathematical calculations based on his wave-equation, the electrons do sometimes forget themselves in this way. As he puts it, there is a finite chance of the charge changing sign after an encounter. You must understand that they only do this in the mathematical problems, not in real life. It seems to me there is good reason for this. A mathematical problem deals with, say, four electric charges at the most; that is about as many as a calculator would care to take on. Accordingly the unfortunate electron in the problem has to make out the direction of past to future by watching the organisation of three other charges. Naturally it is deceived sometimes by chance coincidences which may easily happen when there are only three particles concerned; and so it has a good chance of facing the wrong way and becoming a positive charge. But in any real experiment we work with apparatus containing billions of particles—ample to give the electron its bearings with certainty. Dirac's theory predicts things which never happen, simply because it is applied to problems which never occur in Nature. When it is applied to four particles alone in the universe, the analysis very properly brings out the fact that in such a system there would be no steady one-way direction of time, and vagaries would occur which are guarded against in our actual universe consisting of about 10^{79} particles.

A discussion of the properties of time would be incomplete without a reference to the Principle of Indeterminacy, which was formulated by Heisenberg in 1927 and has been generally accepted. It had already been realised that theoretical physics was drifting away from a deterministic basis; Heisenberg's principle delivered the knock-out blow, for it actually postulated a certain measure of indeterminacy or unpredictability of the future as a fundamental law of the universe. This change of view seems to make the progress of time a much more genuine thing than it used to be in classical physics. Each passing moment brings into the world something new—something which is not merely a mathematical extrapolation of what was already there.

The deterministic view which held sway for at least two centuries was that

if we had complete data as to the state of the whole universe during, say, the first minute of the year 1600, it would be merely a mathematical exercise to deduce everything that had happened or would happen at any date in the future or past. The future would be determined by the present as the solution of a differential equation is determined by the boundary conditions. To understand the new view it is necessary to realise that there is a risk of begging the question when we use the phrase "complete data". All our knowledge of the physical world is inferential. I have no direct acquaintance with my pen as an object in the physical world; I infer its existence and properties from the light waves which fall on my eyes, the pressure waves which travel up my muscles, and so on. And precisely the same scheme of inference leads us to infer the existence of things in the past. Just as I infer a physical object, namely my pen, as the cause of certain visual sensations now, so I may infer an infection some days ago as the cause of an attack of measles. If we follow out this principle completely we shall infer causes in the year 1600 for all the events which we know to have happened in 1930. And at first sight it would seem that these inferred causes have just as much status in the physical world as my fountain pen, which is likewise an inferred cause. So the determinist has me in a cleft stick. If the scientist poking about in the universe in 1600 comes across these causes, then he has all the data for making a correct prediction for 1931; if he does not, then he clearly has not complete knowledge of the universe in 1600, for these causes have as much right to the status of physical entities as any of our other inferences.

I need scarcely stop to show how this begs the question by arbitrarily prescribing what we should deem to be complete knowledge of the universe in 1600, irrespective of whether there is any conceivable way in which this knowledge could be obtained at the time. What Heisenberg discovered was that (at least in a wide range of phenomena embracing the whole of atomic physics and electron theory) there is a provision of Nature that just half of the data demanded by our determinist friend might with sufficient diligence be collected by the investigators in 1600, and that complete knowledge of this half would automatically exclude all knowledge of the other half. It is an odd arrangement, because you can take your choice which half you will find out; you can know either half but not both halves. Or you can make a compromise and know both halves imperfectly, *i.e.* with some margin of uncertainty. But the rule is definite. The data are linked in pairs and the more accurately you measure one member of the pair the less accurately you can measure the other member.

Both halves are necessary for a complete prediction of the future, although, of course, by judiciously choosing the type of event we predict we can often make safe prophecies. For instance, the Principle of Indeterminacy will obviously not interfere with my prediction that during the coming year zero will turn up approximately $\frac{1}{37}$ of the total number of times the roulette-ball is spun at Monte Carlo. All our successful predictions in physics and astronomy are on examination found to depend on this device of eliminating the inherent uncertainty of the future by averaging.

As an illustration, let us consider the simplest type of prediction. Suppose we have a particle, say an electron, moving undisturbed with uniform velocity. If we know its position now and its velocity, it is a simple matter to predict its position at some particular future instant. Heisenberg's principle asserts that the position and velocity are paired data; that is to say, although there is no limit to the accuracy with which we might get to know the position and no limit to the accuracy with which we might get to know the velocity, we cannot get to know both. So our attempt at an accurate prediction of the future position of the particle is frustrated. We can, if we like, observe the position now and the position at the future instant with the utmost accuracy (since these are not paired data) and then calculate what has been the velocity

in the meantime. Suppose that we use this velocity together with the original position to compute the second position. Our result will be quite correct; we shall have made a most unexceptionable prophecy—after the event.

This principle is so fully incorporated into modern physics that in wave-mechanics the electron is actually pictured in a way which exhibits this "interference" of position and velocity. To attribute to it exact position and velocity simultaneously would be inconsistent with the picture. Thus according to our present outlook the absence of one half of the data of prediction is not to be counted as ignorance; the data are lacking because they do not come into the world until it is too late to make the prediction. They come into existence when the event is accomplished.

I suppose that to justify my title I ought to conclude with a prophecy as to what the end of the world will be like. I confess I am not very keen on the task. I half thought of taking refuge in the excuse that, having just explained the future is unpredictable, I ought not to be expected to predict it. But I am afraid that someone would point out that the excuse is a thin one, because all that is required is a computation of averages and that type of prediction is not forbidden by the Principle of Indeterminacy. It used to be thought that in the end all the matter of the universe would collect into one rather dense ball at uniform temperature; but the doctrine of spherical space, and more especially the recent results as to the expansion of the universe have changed that. There are one or two unsettled points which prevent a definite conclusion, so I will content myself with stating one of several possibilities. It is widely thought that matter slowly changes into radiation. If so, it would seem that the universe will ultimately become a ball of radiation growing ever larger, the radiation becoming thinner and passing into longer and longer wavelengths. About every 1500 million years it will double its radius, and its size will go on expanding in this way in geometrical progression for ever.

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800. One day about twenty years ago [I sought advice from] Prof. Landau . . . as to a subject for a thesis. . . . He enumerated some dozen or fifteen topics . . . [and] told me . . . to select one of them, or any other problem of my own choosing, with the single reservation that I should *not* prove Fermat's theorem, an injunction which I have observed faithfully. . . . [Eventually I chose] an investigation of the degree of approximation with which a given continuous function can be represented by a polynomial of given degree. When I reported my choice, he said meditatively, . . . "Das ist ein schönes Thema, ich beneide Sie um das Thema. . . . Nein, ich beneide Sie nicht, aber es ist ein wunderschönes Thema!"—Prof. Dunham Jackson, Preface to *The Theory of Approximation*, 1930.

801. "In the same month [March, 1891] Hardy erected what he called 'The Druid Stone' on the lawn at Max Gate. This was a large block they discovered about three feet underground in the garden, and the labour of getting it from the hole where it had lain for perhaps two thousand years was a heavy one even for seven men with levers and other appliances.—'It was a primitive problem in mechanics, and the scene was such a one as may have occurred in building the Tower of Babel.' Round the stone, which had been lying flat, they found a quantity of ashes and half charred bones."—*The Early Life of Thomas Hardy*, ch. xix., p. 306. [Per Mr. P. J. Harris.]

802. If I had to educate myself again, and had not a living to make, I would leave out a good many things the old dominie thought needful. What was yon awful thing again?—mensuration. To sleep well and eat anything, fear the face of nobody in bashfulness, to like dancing, and be able to sing a good bass or tenor,—that's no bad beginning in the art of life.—Neil Munro, *The Daft Days*, chap. iii. [Per Mr. James Buchanan.]

DIMENSIONS IN GEOMETRY.*

By Prof. A. R. Forsyth, Sc.D., F.R.S.

In selecting a subject for submission to your attention, I have avoided topics belonging to the customary ranges of the school room, the lecture theatre, and the examination arena. My wish is to discuss the notion of dimensions in abstract geometry. In recent years, the "fourth dimension" has been obtruded upon an unsophisticated world in phrases which occasionally defy comprehension. Little more than a year ago, our Sabbath peace was perturbed by a newspaper announcement that a fifth dimension had been discovered, though the explanation which introduced the discovery was little more than incoherent verbiage. Novelists have ventured upon the intangible theme of multiple space: did not one of them, in an attenuated remembrance of Cambridge training, malign a great mathematician by assigning to his genius a fatuous dogma that ghosts are possible by means of the fourth dimension? Theologians of a sort, on both sides of the Atlantic, have not disdained to make a new departure by placing heaven in the fourth dimension: one of them, indeed, has postulated an unlimited number of dimensions as constituting the heaven of heavens, the dwelling-place of The Most High. Nay, coming nearer to our mathematical science, some popular expositions of the successive theories of relativity have stated, almost pontifically as an article in a creed, that Time is the fourth dimension.† Even more bewildering was the rhetoric launched at us some six months ago when we were told, in a metaphorical summary,‡ that the four-dimensional space-time continuum had

> "swallowed up both ether and light and was about to swallow up both the gravitational and electromagnetic fields and corpuscles as well. . . ."

This indigestible banquet would seem a realisation of Prospero's vision:

> The cloud-capped towers, the gorgeous palaces,
> The solemn temples, the great globe itself,
> Yea, all which it inherit, shall dissolve
> And, like this insubstantial pageant faded,
> Leave not a rack behind.

If the quotation were to be continued here, it might conclude with an emended epitaph that "our little life is rounded with a [nightmare]". Being a person of simple mind which prefers not to say more than it thinks it understands, I shall not attempt to clarify these Sibylline oracles.

Again, when newspapers print paragraphs referring solemnly to a fourth dimension, it may have happened to some of you, as certainly it has happened to me, to have been asked for an explanation (of course, "in a few words") of what is meant by the fourth dimension. Let me confess to having sought for an accessible and trustworthy exposition of the fourth dimension, in terms that could be understanded of the people (to use our old English phrase). There is more than enough printed matter; but the results of the quest were not satisfactory and discouraged its further continuance. Thus the great Oxford dictionary, with an air of editorial finality, tells us that

> "the fourth dimension is a property that is to volume what volume is to area or to solids as solids to planes."

The words sound like alternative proportion sums with a common answer. So far as the literal words bear meaning, and if the suggested proportion carries

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* A paper read at the Annual Meeting of the Mathematical Association, January 5th, 1931.

† The representative Relativist (in Eddington's prologue in *Time, Space, and Gravitation*, p. 14) declares: I think that there is a real sense in which time is a fourth dimension—as distinct from a fourth variable. The term dimension seems to be associated with relations of *order*. I believe that the order of events in nature is one indissoluble four-dimensional order.

‡ *The Times*, 17 June, 1930; 30 June, 1930.

its due significance, the implication is that the property thus defined is some space-content of four dimensions: it cannot, on any just interpretation, be a fourth dimension any more than the cited volume or the cited solids can be a third dimension. Even so, a willingness to interpret the words in the most sympathetic spirit must find the declaration meagre in specific quality, either as a definition or as a description.

As a last hope from the outside, I had expected that some repose of mind would be derived from Maeterlinck's fairly long essay on the fourth dimension. Alas! there came only disappointment. He cites the opinions of some famous men. A scientific statement is balanced by a non-scientific extract from Littré's dictionary, so as to lead to a palpable contradiction. There is not a little pleasant reading. But, at the end of it all, I could not make out Maeterlinck's own conception of a fourth dimension and was obliged to subside under the author's frank warning *

> Ne croyez pas qu'après avoir lu [cet essai] vous saurez ce qu'est la quatrième dimension.

My aim to-day is unambitious. Leaving on one side all the high-flown declarations, and being unable to accept (*ex animo*, to use the ecclesiastical phrase) some of the over-iterated but unestablished dogmas, I shall try to set out what ordinary mathematicians like ourselves mean by dimensions, what we mean by a fourth dimension in geometry, and how we can thus proceed quite simply to four-dimensional geometry. It is my hope that the explanations will reveal these dimensions of lowest order as the prelude to further dimensions in abstract geometry.

I.

Probably it will be conceded that space in some form appeals to all human consciousness, and that some notion of space is common to the thought of all minds though its possession may convey a vast variety of suggestion. Ideas concerning the nature of space gradually acquire some qualities of definiteness, after the earliest perception of differences of position, of distances, of sizes in a general sense. Such ideas are based upon conceptions stirred by instinctive observation of an external world surrounding the observer. Gradually, a more precise but still undefined notion of extension, as common to distances and to sizes, ceases to be rather difficult to grasp, becomes less vague as a common property and more amenable to thought, though only after continued reflection which need not always be actively conscious. Ultimately some not inadequate idea of space is acquired, abstract though not fully recognised as abstract; it is suggested initially through instances of measurements and estimates of what we call the real and the concrete: yet that idea of space, received, it may be, with some hesitation and timid slowness, is held to be distinct from the space itself in which the real and the concrete are held to exist. The mind thus acquires an idea of abstract space, corresponding indeed in its early stages with the experiences of reality, yet completely different in character from the space in external reality and, most significant for our purpose, not subject to all the restrictions and all the empirical difficulties imposed by the external reality. When that stage is reached, we have obtained a notion of extension in general. We no longer restrict thought to particular kinds of extension, as illustrated by familiar types. We can argue about extension in the abstract, about ideal extension, without any need or any practice of reverting to concrete examples, to check or to verify the results of reasoned argument.

During this process of absorbing knowledge, one faculty is beginning to be exercised though its exercise is not always consciously noted. We grow

* *La vie de l'espace*, p. 9.

accustomed to discriminate between the real and the abstract, between the concrete and the ideal, even when they are described by the same word. We keep them in unharnessed correspondence; yet we are not hindered from progress in either, merely because progress in the other cannot be achieved. We can dream dreams and we can see visions, though we cannot realise projects. Made stronger in power by the growing exercise of the faculty, the meditative mind, if only to preserve the clearness of its own reasonings, will be on its guard against that tendency to mental slackness which occasionally, in argument, confuses correspondence with identity, and, because of a common term, interchanges the absent real and the latent ideal as though they are equivalent. It has happened in the past, and doubtless will happen in the future, that such tacit interchanges have been made in mathematical arguings which have thereby been led into strangely untenable positions. But there is a converse peril. We have notions, derived initially from sensation and from experience: they are combined, generalised, developed, in our thought; we argue, we construct, until the modified and extended ideas lead us into what Sylvester delighted to term a "new world" of knowledge. But there is no inherent necessity that a development of the ideal in the realm of abstract thought should have, always, a corresponding counterpart to be found in the concrete externals which suggest the initial ideas. Thought is not always the harbinger or the slave of circumstance.

So it is with space. There is one space as conceived in thought: there is another space as experienced by sensation. The one has the character of a theory: the other is of the nature of practical science. The one belongs to pure mathematics: the other belongs to physics—let me use an old-fashioned phrase—to the nature of things. They are different from one another: they begin by being related to one another, but they diverge: and there comes a stage when panting experience toils after thought in vain. We thus are obliged to discriminate between the ideal and the real in all, or nearly all, argument about space. So the geometry of our thoughts, whatever be the logical scheme of geometry, is abstract, is a science concerned with what is ideal; it has not remained, as it began among the ancient Greeks, an applied science concerned initially with the concrete. Even in our day, the corrections made in geodetic measurements are the outcome of abstract geometry.

Now when we mention a length, or a breadth, or a height, usually we are referring to a fairly specific direction of measurement made in external space: it is concerned with some reality. But the practical concern with reality may become evanescent during calculations: only the general ideas of all the measurements survive as ideal magnitudes from which all specific quality has parted. The measurement has come to be what, vaguely at first, we call a dimension; and so the use of the term dimension arises as a sort of generalisation of linear measurement when thought effects a transition from the specific real to the generalised ideal.

But this word dimension (like so many words of ordinary parlance turned to mathematical use, such as class, degree, order) is made to bear a variety of meanings, each dependent upon the circumstances in which it is used. We speak of the dimensions of a ship: these can be made to include the draught at full load as well as the depth, can even include the horse-power of the driving machinery. We speak of the dimensions of an algebraical quantity, as estimated from its constituent components. We speak of the dimensions of a physical magnitude, as estimated through standard magnitudes to which it has precise relations. Yet in speaking of the dimensions of a ship by a single word while several different quantities are included, and of the dimensions of an algebraical quantity, and of the dimensions of a physical magnitude, we are using the word solely for brevity of description. If challenged by a punctilious critic, there would be an immediate acknowledgment of the fact that the word dimension, so used, does not bear the same significance as when it is used to

imply purely geometrical measurements such as length or breadth or height. There is convenience in the practice, so long as there is no confusion: but the dimensions of a physical magnitude often are different, some are entirely distinct, from the dimensions of a geometrical measurement. For our immediate purpose to-day, the term dimension bears its customary meaning and denotes the abstract idea suggested by linear measurements in geometry.

II.

You may have observed that in this statement the word "linear" has been introduced. Before proceeding further, it is desirable to render as precise as possible the meaning to be attached to the word "line" and to cognate derivatives. There will be a concurrent advantage, in so doing; it will emphasise the fact that our working conception of geometry is ideal, distinct from all practical applications.

We know what we mean by a line. We think we know what we mean by a straight line: if challenged, we might declare it to be—what obviously it is. But there are many obvious things which cease to be either simple or clear when scrutinised with care; and if a persistent inquirer asked for a more illuminating account of a straight line, what is to be the answer to his demand? Perhaps some would offer the old Greek answer, descriptive but rather vague, that it is a line lying evenly between any two of its points. Such a description is not selective enough: the description is completely true of any part of a great circle on a sphere, such as a meridian of longitude. Others, perhaps, would describe it as the shortest distance between any two points; but such a shortest distance is affected by the means of passage between the points, and the description would cover a stretch of direct railway line and might even cover some parts of the string on a well-tied parcel. Others, again, might describe it as the result of producing a direction, without change, ever so far in both senses: a somewhat hypothetical answer, because nobody has ever attempted to produce a direction "ever so far"—it may be from lack of will or of leisure, it may be because the way is barred by physical difficulty, and the result has never been known. A more sophisticated answer has been that it is the path of a ray of light in a vacuous space: hardly satisfactory as preliminary explanation, partly because it is an inference from a speculative physical theory which doubtless has implicitly assumed times without number the notion of a straight line, partly because it contributes rather than receives information about the path of the visionary ray of light. Moreover, much of the modern theory of our universe, and all the successive forms of all the theories of relativity whatever be their respective differences, combine to declare that the path of a ray of light is very definitely not what we commonly call a straight line.

Nor is more trustworthy help to be found when it is sought from more familiar circumstances. We often declare a straight line to be the pencil line drawn carefully along a perfect straight-edge ruler; but the drawn mark is not a line at all, it has slight breadth as well as length, and in fact is a long narrow area. We might declare the edge of our perfect ruler to be the desired straight line; but under the detective eye of a microscope, the edge reveals surprising deviations from what we judge to be continuous accurate straightness. Again, the less simple conception of rotation has been invoked: a line is straight when, round it as an axis, a body can rotate without change of shape of itself or of the axis. Alas! a smoke-ring from a friendly pipe, a steam-ring puffed from the funnel of a starting locomotive, would show that, even initially, the definition is of no avail. Still hankering after a definition that shall be based upon trusted knowledge, we might (as a last desperate attempt) describe a straight line as one which has no bending or, if we are ready to delude ourselves by a more professional phrase, as a line which has no curvature of any

kind. If such a statement bestows any illumination, the gift need not be grudged: to me, it is unveiled tautology.

The actual fact is that, in submitting these alternative answers in their succession, we have been appealing silently to practical experience and have been tacitly expecting it to provide, either in full or in part, something that shall help us to formulate our clear but elusive idea. Somehow, and rather unconsciously, we have distilled from notions, suggested by our experiences, an ideal conception which is beyond the range of verifiable reality. Somehow (perhaps the psychologists can divine the secret of the process) we have combined various notions, some of which are immediate, and some of which are the indirect result of reflection. At any rate, we have attained the idea of a straight line; if we cannot frame an adequate formulation in express words, we can at least postulate the idea of linearity as an essential quality of the undefined conception of direction, and we postulate it frankly, being in possession of various contributory notions. A first beginning of our geometry was made in the conception of ideal or abstract space; a further stage is attained through the acquisition of the idea of straightness for a line, abstract, outside all memories and all impressions of what is material in experience. In attaining that idea, unrecognised or unacknowledged assumptions may have been made with unsuspected profusion; for their revelation and their exposition, we must ask for the help of the logicians, appealing for tenderness of guidance from their critical analysis. Though we may not be able to provide a logical definition that will weather every blast of criticism, still (to use a phrase which I heard Fitzgerald use in like circumstance) we "feel in our bones" that we do conceive an idea of abstract space having unrestricted extension, and we do conceive an idea of abstract straight lines having their special characteristic property.

These ideas have none of the hampering restrictions of real apparatus, none of the trammels that bind physical experiment. Our thought has full liberty to use its own freedom in dealing honestly with such ideas in a realm that is all its own. From time to time, as may be convenient for our purpose, we may return to the range of the real and the concrete. In particular, we shall use the power to make diagrams which shall be a more or less rough representation of our notions at the moment, which may indicate some correspondence with actual reality and which may even provide approximations to ideal accuracy. But such diagrams are only illustrative subsidiary aids; they do not constitute our ideal geometry any more than a speculative theory of nature is a substitute for nature.

III.

Now let us proceed to utilise these ideas of space and line, combine them, make inferences from them. Thus far, in our conception of space, no restriction of any kind has been imposed. Whatever else may be its character, position in the space is freely at our choice without any reserve. Position (that is, position devoid of magnitude) will be denoted by the term point, that is, an ideal point, not the rather gross dot which, in our drawn diagram, is our feigned approximation to position.

But what is the relation between space and line, between the all-copious space and the ideal line? I shall assume (and it is a very large assumption) that our ideal space on the grandest scale contains every ideal line that we can conceive. Obvious, perhaps you will think, after a moment's reflection: surely every point on the line is a position, and space on the grandest scale should contain every position conceivable. But take critical examples which are at our command. On any very large sheet of paper, the assumption would seem to be justified in a gross non-ideal sense: we can draw upon the sheet an unlimited number of what we call straight lines. But if the space were

the surface of a school-room globe, however large, no straight line can be drawn in that space: were the globe the earth itself, straight lines cannot be drawn upon the surface, for all terrestrial lines are curved. So, from the very beginning we make the assumption, and we recognise it to be an assumption, that our ideal space * is such that it contains all the ideal lines we can conceive. I ask you to allow me to assert dogmatically (from other considerations which lie far beyond the range of an hour's discourse) that there are all kinds of spaces for which the assumption is not justified and that a space, specified by this particular assumption, has the simplest properties among all the ideal spaces that we conceive. Because of its relations with straight lines, it is called a linear space, or an uncurved space, or a Euclidean space.

Accordingly, our ideal linear space contains all the straight lines which thought can conceive. On any line there are positions which are unlimited in number. The characteristic property of straight direction in the ideal line leads, by a simple calculation, to the mathematical result that a knowledge of any two points on the line is sufficient for the complete unique determination of the line. When therefore we select any two different positions in space, these being represented by points, we have potentially chosen a straight line which is contained entirely within the space.

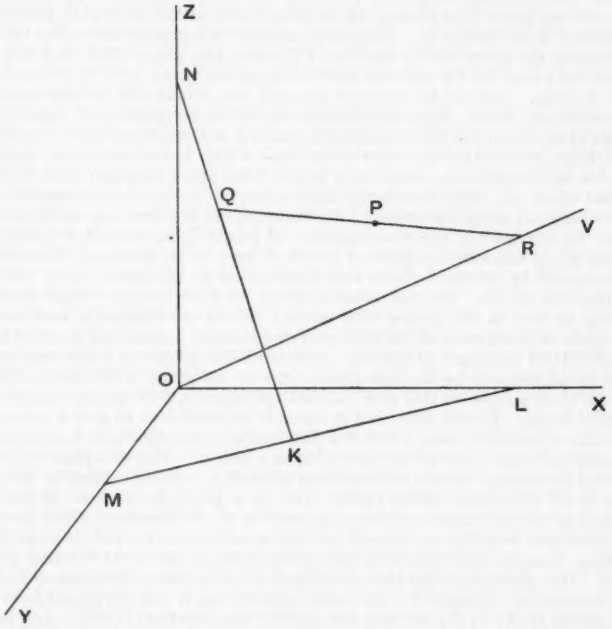
Moreover, at every point in ideal space, any assigned direction determines a straight line and determines only one straight line: there cannot be two straight lines through one point with the same direction. There can be any number of curves, passing through the point in an assigned direction at the point; they all touch the one and only straight line through the point in that direction.

IV.

Now take any point O and, through it, conceive a line OX drawn in any direction: or if we begin differently, take any point X different from O and conceive the line which is drawn to join the two points O and X. Through that same initial point O, but in any direction different from OX, conceive another straight line OY drawn in the linear space. On OX take any point L, and on OY take any point M: the points L and M are any points whatever on the respective lines, subject solely to the negative requirement that each of them shall be distinct from O. Because they lie on different lines, L and M are distinct points; hence we can conceive the straight line LM to be drawn, and it must lie in our ideal space. The point L on OX can lie anywhere within OX, or beyond X, or behind O; and the point M on OY can lie anywhere within OY, or beyond Y, or behind O. On the line LM, let any point K be chosen anywhere along its direction, whether within LM, or beyond M, or behind L. By a simple algebraical calculation, it is easily proved that the line OK meets a line XY drawn to join XY, that the line XK meets OY, and that the line YK meets OX. Thus imagining any number of points L and M, chosen independently of one another (that is, by two independent choices), and effecting the constructions which have just been specified, we have an unlimited number of lines meeting one another. Just as we take all the points on a line and say that they constitute a locus, so we take all the preceding lines and say that they (as well as all the points on all of them) constitute a locus; and, following a suggestion derived from practical experience which happens to be a guide, we say that this new locus is a plane. This plane has become completely and uniquely definite by means of the two different directions OX and OY through O or, if we choose so to state the facts, by means of three points O, X, Y in our linear space. In obtaining this plane from unhampered initial choice, no limitations and no restrictions have been

* Such an ideal space is called sometimes *flat*, sometimes *homaloidal*: it is unnecessary, at this stage, to amplify the vocabulary of technical terms; and neither term will be used.

imposed; beyond the assumption of the linearity of our all-comprehensive space, no limitations have been imposed upon the ideal space; and no new quality has been assumed. Thus we have a plane locus; the plane, being settled by three points O, X, Y, can be called the plane OXY. Each selection of three points such as O, X, Y—that is, such that no one of them lies on the line joining the other two—determines a plane. Each of them is a locus of straight lines: each of them contains an unlimited number of straight lines. Each of them is a plane such as Euclid propounds, the kind of plane with which Macaulay's "every schoolboy" (in these days, he would have added "and every schoolgirl") is familiar.



Further, every plane contains an unlimited number of lines in an unlimited variety of directions within itself. Take any two points A and B in the plane; and imagine two lines, one through A and the other through B, drawn in the plane and having the same direction. An easy calculation leads to the conclusion that two such lines do not meet one another at any finite distance from A or from B, however far we might proceed along the lines. Two such lines are said to be parallel: the term implies merely a renewal of a customary notion of parallelism: and, in fact, two lines are defined thus to be parallel when they have the same direction in the ideal space. Moreover, the property of linear space which has already been stated in the declaration that, through any point there passes only one straight line with an assigned direction, carries the property that, through any point, there is one and only one straight line parallel to a given line.

V.

Now let us move further afield in the ideal space. Take, again through the initial point O, any direction OZ which does not lie in the plane OXY. The point Z is any point along the direction distinct from O; as OZ does not lie in the plane OXY, the point Z does not lie in that plane. (The two lines OX and OZ of course determine a plane OXZ: equally of course, the two lines OY and OZ determine another plane OYZ. These new planes, like the plane OXY, lie in our comprehensive ideal space; but they are not our immediate concern). Along the new line OZ let any point N be chosen quite freely, either within the part OZ, or beyond Z, or behind O. Then, coming back to the point K in the earlier construction, imagine N joined to K by a line; and along this latest line, let any point Q be chosen, again quite freely, either within the piece KN, or beyond N, or behind K. Once more a simple calculation shows that the line XQ meets the plane OYZ, the line YQ meets the plane OZX, the line ZQ meets the plane OXY; and the line OQ meets the plane XYZ if we construct such a plane. (Do not be mentally beguiled into thinking it obvious that any line meets any plane; it is a beguilement due to the temptations of experience: in our ideal space, the full extension of which is not restricted to the results of experience as revealed by observation, and which is as extensively comprehensive as thought can conceive, a line in ideal space need not meet a plane in that space any more necessarily than a telephone wire above a street meets a tramway rail along the street.) To return to our construction at its present stage, we can take an unlimited number of points L, an unlimited number of points M, an unlimited number of points N, each in an absolutely free choice, unhampered by previous choice and unrestricted by provident care; there is a three-fold choice. By our constructions, we have obtained lines meeting planes, as well as the planes themselves; we have obtained a vast settled aggregate of lines, each of the lines carrying its own points, and thus we have a vast settled aggregate of points. Just as all the points on a line make up a locus (it happens to be the line itself), just as our plane is the locus of lines and all their points, so this new vast settled aggregate of points constitutes another locus. To this new kind of locus, it is convenient to give a name, for the sake of brevity; and I call it a flat (not a hyperplane nor a superplane, because the locus is not of the same kind as a plane). Just as a plane could be covered by straight lines in all directions in itself, so a flat is riddled by straight lines in all directions within itself. Just as a plane is not bent or curved, because of straight lines everywhere in itself in all its directions which prevent any chance of bending out of itself, so a flat is not bent or curved, because of its riddling straight lines which equally prevent all chance of its bending out of itself. Our typical flat has been developed from the three directions OX, OY, OZ, meeting in the point O; we could equally regard it as developed from the four points O, X, Y, Z; we therefore specify it as the flat $OXYZ$. Any point in our ideal space can be taken as the initial point O; any other three points X, Y, Z, can be taken provided the directions OX, OY, OZ are distinct and do not lie in one plane. Consequently any four points in the space determine a flat; as these can be chosen in an unlimited multitude of ways, each way providing a flat, so we can have an unlimited number of flats in our ideal space.

Thus, in our space, we have planes and flats as well as straight lines. Every plane is crossed by such lines: every flat is riddled by such lines. No line is curved: no plane is curved: no flat is curved.

We can make other adventures forth, into the ideal space of our conceptions. The ideal has one advantage over the real: when our concern is with the ideal, we are bound to obey only the restrictions of reasoned thought, whereas we are troubled with the hard conditions of circumstance when our concern is with the real and the concrete. When actual possibilities can help, we shall use material aids to assist thought. Indeed, such aid has already been

accepted: our diagram is drawn on a plane (or what passes for a plane in material representation): yet it already purports to represent three directions, OX, OY, OZ, which do not lie in one plane. In fact, imagination and perspective combine, under the spell of an artist, to represent the familiar solidities of nature on the surface of a canvas; and we can adopt his working principle.

VI.

So, as our next adventure into ideal space, let us start again from O and proceed in a direction OV which does not lie in the flat $OXYZ$. As OV does not lie in the flat already known, the directions OX, OY, OV, do not lie in one plane: they determine a flat $OXYV$. Similarly the directions OX, OZ, OV, determine a flat $OXZV$; and the directions OY, OZ, OV, determine a flat $OYZV$. Finally, we note that the four points X, Y, Z, V, which do not lie in one plane (because V is not in the flat $OXYZ$ and therefore not in the plane XYZ), determine a flat $XYZV$. All these flats are distinct and all of them lie in our ideal linear space.

Along OV take any point R with an unfettered choice: R may lie within the portion OV of the line, or beyond V, or behind O. We utilise all our previous scaffolding and increase it, by imagining this new point R joined to Q by a straight line which of course lies in our linear space. Along this line QR, we take any point P, again with an unfettered choice, within QR, or beyond R, or behind Q. By a simple new calculation of the same kind as before, we find that the line OP meets the flat $XYZV$; that PX meets $OYZV$, that PY meets $OXZV$, that PZ meets $OXYV$, and that PV meets $OXYZ$. Each line meets its counter flat in a point and therefore meets every line passing through that point in the flat. We thus have an unlimited new system of lines (and therefore also a further system composed of all the points on the lines) in a definite aggregate. Exactly as the aggregate of lines and points constructed from two directions OX, OY, constituted the locus called a plane, and as the lines and points constructed from three directions, OX, OY, OZ, constituted the locus called a flat, so our new definite aggregate, constructed from four directions OX, OY, OZ, OV, constitutes a locus. As a name may be convenient for brevity, let it be called a domain—at the moment, I cannot think of a familiar word that shall describe its linear constitution in the same kind of way as do the terms plane and flat. The domain originates through a four-fold choice of distinct directions OX, OY, OZ, OV, which do not simultaneously lie in a flat: it may be described the domain $OXYZV$. Its nature is such that, through every point in its range, a straight line can be drawn in any direction which lies in the domain. The domain is not bent or curved.

Within our ideal linear space of unrestricted range, we now have lines, planes, flats, domains. A line can be drawn in any direction; there is a single (one-fold) choice. A plane arises from two different directions; there is a double (two-fold) choice. A flat arises from three different directions, not in one plane; there is a triple (three-fold) choice. A domain arises from four different directions, not in one flat; there is a quadruple (four-fold) choice.

In this succession of ranges, each type of range is more extensive than its predecessor. A plane is more extensive than a line; for the plane contains two directions from which constructions are made whereas the line has only its own direction. A flat is more extensive than a plane; for the flat contains three directions from which constructions are made, and these three must not lie in a plane. A domain is more extensive than a flat: for the domain contains four directions from which constructions are made, and these four must not lie in a flat.

VII.

I trust that, on the one hand, the notion of our linear space, in its comprehensive plenitude, is clear in thought, and that, on the other hand, the con-

ceptions of the successive ranges as constructed also are clear in thought. If so, it is manifest that further progress in the construction of more extensive linear ranges is possible by successive stages. Is there no end to the progressive process? Effectively we can arrest our progress: for this purpose, let us examine the successive types of range a little more closely, and consider the line, the plane, the flat, the domain, in particular. During this consideration we must keep clearly in mind the discrimination between the ideal abstract space of our thought and the real external space of our sensations. The warning is all the more necessary because, at first, there are close correspondences between the ideal space and the real space. Timid thought is apt to demand confirmation from experience, almost at every step with some thinkers: for our purpose, there is an easing of constructive thought if we regard empirical knowledge as incidental illustrations of our conceptions and do not hitch our conceptions to the tail of our experiences. The spirit must not falter because the guiding hand is neither felt nor seen.

We begin with a line, such as OX. A line has extension along itself alone, in both senses of its direction, backwards and forwards in the customary phrase. Its extension is limited to the single prescribed course, and can be described as one-fold. Introducing the term "dimension" in its geometrical sense, we say that a straight line is of one dimension; we even can say that a straight line is a space of one dimension. It is not the only space of one dimension; any curve is also such a space. There is, however, no necessity of encumbering ourselves with unnecessary baggage in our present march: let it suffice that we have attained the conception of a space of one dimension. A straight line is the selected representative of such a space. It lies in our plenary ideal space.

Consider, next, a plane, such as the plane OXY which was constructed from two distinct guiding lines OX, OY. The plane is crossed by all sorts of straight lines, each with its own extension; thus there are all sorts of extensions in a plane. But every extension in the plane can be composed of one extension, along or parallel to the direction OX, and of another extension, along or parallel to the direction OY. These two directions, distinct from one another, are independent of one another: each of them proceeds absolutely free from any influence of the other. The whole extension in the plane can be described as two-fold: it is associated with the two-fold choice of a couple of initial guiding lines such as OX, OY. Again using the geometrical term "dimension", we say that the plane has two dimensions: we can say that a plane is a space of two dimensions. It is not the only space of two dimensions; any familiar surface of our experience is a space of two dimensions. We have, however, attained the conception of a space of two dimensions. A plane is the simple selected representative of such a two-dimensional space. It lies in our plenary ideal space.

Next, consider a flat, such as the flat $OXYZ$ which was constructed from three guiding lines OX, OY, OZ. The flat is riddled by straight lines in all sorts of contained directions, each with its own extension; thus there are all sorts of extensions in the flat. But every extension in the flat can be compounded from an extension in the two-fold plane OXY or parallel to that plane, and from an extension along or parallel to the direction OZ. Every extension in the plane can be compounded of an extension along or parallel to the direction OX and of another extension along or parallel to the direction OY; and therefore every extension in the flat can be compounded of three different extensions, which respectively lie along or are parallel to the three directions OX, OY, OZ. These three directions are distinct from one another; they are independent of one another; each of them proceeds absolutely free from any influence of the other two, The whole extension in the flat can be described as three-fold: it is associated with the three-fold choice of a triad of initial guiding lines such as OX, OY, OZ. Again, using the geometrical term "dimen-

sion", we say that a flat has three dimensions; we can say that a flat is a space of three dimensions. It is not the only space of three dimensions; thus the space restricted to the strands of a thick twisted rope suggests such a space. We have attained our conception of a space of three dimensions. A flat is the simple selected representative of a three-dimensional space. It lies in our plenary space.

Now proceed into a domain, such as the domain $OXYZV$ which was constructed from four guiding lines OX, OY, OZ, OV, not all lying in one flat and chosen originally with absolute freedom. In this domain there are all sorts of extensions. But every extension in the domain can, by its construction, be compounded of an extension in the flat $OXYZ$ (or parallel to such an extension) and of an extension along or parallel to the direction OV. We have already seen that every extension in the flat $OXYZ$ can be made up of three different extensions which respectively lie along or are parallel to the three directions OX, OY, OZ. Consequently every extension in the domain can be compounded of four different extensions which respectively lie along or are parallel to the four directions OX, OY, OZ, OV. Of these four directions, the three, OX, OY, OZ, are distinct from one another and are independent of one another: that same triad led to the construction of the flat $OXYZ$; and the fourth of our directions, OV, was taken as some direction outside the flat, so that extension along OV is not resoluble into extensions along OX, OY, OZ. The direction OV is independent of OX, OY, OZ. The four directions OX, OY, OZ, OV are distinct from one another: they are independent of one another: each of them proceeds absolutely free from any influence by the other three. The whole extension in the domain can be described as four-fold: it is associated with the four-fold choice of a tetrad of initial guiding lines, such as OX, OY, OZ, OV. Once more using the geometrical term "dimension", we say that a domain has four dimensions; we can say that a domain is a space of four dimensions. It lies in our plenary ideal space.

A domain is not the only space of four dimensions. But, at this stage in our progress, we have to face an ending of the correspondence that hitherto has been possible to remark between the ideal of thought and the actual of experience. It has been possible to cite recognisable examples of spaces, of one dimension different from a straight line, and of two dimensions different from a plane; it is not possible to cite an example of a space of four dimensions, whether a domain or a range different from a domain, which occurs as a familiar phenomenon.

VIII.

I know that a whole school of philosophers would cite "events" in this connection, prescribing of course the true significance for the term "event". Now, however, it may be defined for this use, an "event" occurs in a flowing succession of time at a spatial position. There are three variables, usually, to specify the spatial position. There is a single variable to specify the time. Formulae are framed "on a slender empirical basis" * which involve simultaneously the three space-variables and the single time-variable. This aggregate of four variables, not all of the same kind, becomes (under the repeated iteration of some of these philosophers) a space-time continuum of four dimensions: it has even been described as "indissoluble". To my thought, this attempted intellectual fusion of heterogeneous elements is devoid of coherent meaning. Remember that the combination of heterogeneous variables to specify an event is not a discovery of yesterday, however it may be decked in modern phrase: it has served its due purpose ever since astronomy became a science. But time, by its nature, and through its relations, and from every idea which it stirs whether concerning phenomena or in thought, is essentially diverse from any dimension of space and is not fusible into a continuum

* Einstein, *The Times*, 5 February, 1929, p. 15.

with dimensions of space. To me, the density of a bubble of a gas in a fluid or of a distant mobile star, the energy of a moving body, the temperature of a surrounding medium, all equally alien to geometry, are not more impossible as a dimension to be fused with the homogeneous dimensions of space, than is time. Indeed the claim, for fusion of heterogeneous entities, seems to me not unlike Glendower's claim:

I can call spirits from the vasty deep,

met adequately by Hotspur's questioning retort:

Why, so can I, or so can any man:
But will they come when you do call them?

Personally, I find no difficulty in conceiving successive dimensions beyond the third in ideal space, without the aid of citable instances from experience; it seems just as easy, as it is to discuss the geometry of Euclid from diagrams, without persistent reference to the dimensions of a regular brick. There is the familar practice of using phrases "backwards and forwards", "right and left", "up and down"; we might try "in and out" of the space of experience to specify some notion of a fourth dimension, but it is superfluous. Clearness and simplicity in ideal geometry are best attained by discarding, at as early a stage in conception as possible, all suggestions of correspondence with impressions of the external and the observable. So the net result is that, without the help (may I add, without the necessity?) of a citable example to stimulate suggestion, we have attained the conception of a space of four dimensions as represented by what has been called a domain.

IX.

We have our notion of four-dimensional space. The way is clear for the mental construction of spaces of more dimensions still. Is there no end to this graded succession of multi-dimensional spaces? We can call a halt when we will. Recall the process which has been adopted on each occasion when a space of an increased number of dimensions was framed. When, in the mind's eye, a two-dimensional plane was framed, a choice was made of a direction distinct from the preceding one-dimensional line. When a three-dimensional flat was framed, a choice was made of some direction outside the preceding two-dimensional plane. When a four-dimensional domain was framed, a choice was made of some new direction outside the preceding three-dimensional flat. And so on, in succession; in every instance, the construction of a new kind of space, one dimension ampler than its predecessor, has depended upon the actual and active selection of a new direction not contained within the space already framed. Were no such other direction possible, there then would be an end to the types of space constructed according to the successive increase in the number of dimensions. It is the kind of end that might be imposed—I use the word "might" and not the word "would" or the word "must"—by our experience of what was provided by our sensations. Again, we can imagine that, when once a four-dimensional domain is attained, there would be no direction lying outside the domain; there would be no further directional possibilities in our ideal space. The domain would be the whole of space; the plenary ideal space would be four-dimensional. But there is another action that is freely permissible, and here lies one advantage of ideal thought over crude circumstance. We can always avoid proceeding further, by declining at any stage to consider new directions not already included at the stage attained. We could call a halt when a two-dimensional space is attained; the simplest of such spaces is a plane; and we are free to consider plane geometry in its fullest detail. We could call a halt when a three-dimensional space is attained: the simplest of such spaces is a flat, which without hesitation we take to be what we call "ordinary" space; and we

are free to consider solid geometry in its fullest detail. Similarly in an arrested space of four dimensions—we may take a domain being characterised by Euclid's axioms as the simplest of all such spaces—the geometry can be developed, of course with the ampler possibilities provided by new configurations, just as completely as the geometry of curves in a plane and of surfaces in triple space.

We need not stop at four-dimensional space. We need not continue without cessation. All reasonable demands are met by considering a linear space of n dimensions attained by our progressive process, where n is any integer that remains as unspecified as it is in the binomial theorem. There is some knowledge—it is far from complete—of the properties of configurations which occur in that general multi-dimensional range. If a desire for precise detail should arise, the desire can be satisfied by a specification of the integer n. If there is a willingness to let thought roam at large through multiple space, we may attain to Maxwell's description of Cayley:

> Whose soul, too large for ordinary space,
> In n dimensions flourished unrestricted.

X.

In conclusion, I should like to submit, to the consideration of those who are anxious to discover some external counterpart of a fourth dimension, a notion that has recurred to me in recent years, not once or twice only. As you know, the ancients believed the earth to be flat: the geometry of the surface of the earth was Euclidean plane geometry; the three angles of a triangle are equal to two right angles. When the earth was found to be not flat, straight lines could not be drawn on it; and the Euclidean geometry, though an effective approximation on a small scale, was no longer adequate for large measurements. In place of straight lines as sides of triangles, terrestrial shortest distances were the subject of measurement; these shortest distances are geodesics, still lying in the two-dimensional surface: but they are curves. But in the mathematical conception of curvature, the curvature is the measure of the rate of deviation from straightness; to estimate that measure, we must imagine means of providing the straightness, which is non-existent on the surface of the earth. The straightness must be provided outside the surface of the earth. Happily for ease of conception, that surface exists in a space more extensive than itself, in the ordinary triple space of experience. In that triple space, straight lines in all directions seem obviously possible: indeed, any notion that straight lines in any direction are not possible, that ordinary space is curved, would seem as contrary to fact and thought, as a notion of a curved terrestrial surface would have seemed to the ancients. Still, there is curvature of the earth's surface; and there is an external space, more extensive than the terrestrial surface, by reference to which the curvature of the terrestrial surface is measured.

Now for more than half a century, thinkers have been exploring the subject called relativity. Less than twenty years ago, some theories had taken definite form. One of these, associated with the name of Einstein, was the first to provide an explanation of an unexplained phenomenon connected with the planet Mercury. So that theory, due to Einstein, demanded attention, may be said to have absorbed attention. Other developments followed. Modifications of the theory, amounting to modified theories, have come with the thickness of the falling leaves of autumn. But through all the theories in all their forms, one inference persists: it is that, within the utmost limits of observable space, the geodesics on the grand scale are curves and are not straight lines. The deviations from straightness are minute, on such physical scales as can be manipulated by man; but according to the theories, which

claim to interpret cosmical phenomena, there are such deviations. (Just a word of warning, in passing. The observations are of minute quantities; the differences among these small quantities, though small of course, are relatively grave; and, to my mind, there is need of a fundamental revision of the treatment of the small observations with not insubstantial relatively small differences. But that is another question.) To return, it must not be forgotten that theories are no more than human interpretations of human observations, on the basis of knowledge possessed at the time of their promulgation; and inferences from such theories are not established certainties in nature. We cannot, therefore, be certain that, on the grand scale, the triple space of our experience is curved. But the inference of curvature is one which undoubtedly demands consideration. Let us assume, for the moment, that we may consider our cosmical space, the triple space of experience, to be curved. The mathematical conception of curvature again recurs, as it came in association with the curvature of the surface of the earth. To frame an ideal estimate of the curvature of cosmical space, straightnesses are mathematically necessary: they must be external to the triple space of cosmical phenomena, just as there had to be a triple space more extensive than the surface of the earth, by means of which the curvature of terrestrial measurement was estimated. In this regard, a curvature of cosmical triple space would make a mathematical demand for a space more extensive than itself, a demand for a space of at least four dimensions. But that space would be a geometrical space, not a heterogeneous continuum: the dimension or dimensions, supplementary to the three already accepted, would be of like nature with those three: it, or they, would not be time, would not be entities of a non-geometrical character. That ideal space, as demanded by mathematical conception in order to estimate the highly speculative curvature of triple cosmical space, would certainly involve a fourth dimension, additional to (and of the same nature as) the three dimensions with which we deem ourselves familiar.

There remains the question which is of human interest: even if our mathematical calculations thus imply a necessity for an ideal fourth dimension at the least, is there any real counterpart that can be detected in the universe? No man is in a position to propound a declaration, on observations that are convincing or on authority that can be trusted. The reasoned development of ideas has led us far beyond the observations which started the ideas, has led us to other notions that may or may not be susceptible of confirmation by other observations in the future. The hope or the desire for such a confirmation may prove as vain as other human wishes which aspire to the unattainable. For the present at any rate, our multi-dimensional space is a purely mathematical conception, as ideal as is the ideal Euclidean space. Some such correspondence, in whole or in part, may ultimately be found in the phenomena of what we call reality. Yet, even if such correspondence should never emerge into recognition, the whole theory of that multi-dimensional space remains an inalienable part of the realm of constructive thought.

A. R. FORSYTH.

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803. Mathematics was the business that ruined me—mathematics, that last refuge of the half-wit. I have no word low enough for this miserable, stupid, utterly inexcusable insult to the intelligence called mathematics, which so neatly excludes all possible attempts to use the brain. (This peroration is partly inspired by the fact that I could never figure out how long it took those delightful asses A, B, and C to fill the cistern with wall-paper, and didn't much care anyway.) . . . Besides, there were so many fascinating books to read, and so much fascinating devilment to get into, that it was difficult to become passionately excited over the fact that the square on the hypotenuse is equal to the sum of the squares on the other two sides.—John Dickson Carr, in *Books of the Month*.

THE REPORT ON THE TEACHING OF MECHANICS.*

THE **President** (Professor Sir Arthur Eddington, D.Sc., F.R.S.), on taking the Chair, said: A most important activity of our Association during the past year has been the production of the *Report on the Teaching of Mechanics in Schools*, which is the subject for discussion during the first half of this morning. I will ask Mr. Robson to open the discussion.

Mr. **A. Robson**, in opening the discussion, said: The *Report* which we are to discuss this morning is not the first Mechanics Report published by the Association. An earlier Report is contained in the *Gazette* of December 1918, which consists of eight pages, together with three appendices—about 25 pages in all—and contains alternative schemes for courses of elementary mechanics. That *Gazette* went out of print three or four years ago, and in 1927 the General Teaching Committee appointed a sub-committee to prepare a new Report; a few general principles were laid down in advance for the sub-committee to follow, *e.g.* "fads" were to be avoided at all costs. Some of the material of the old Report was used, and the sub-committee also had the advantage of hearing the views of two or three teachers from outside the Committee. Also, when the draft of the new *Report* was approved by the General Teaching Committee in November 1929, some valuable help was given towards finishing it off by two or three members who did not belong to the sub-committee. Help was also received for an appendix.

In the main, however, the *Report* is the work of the sub-committee. Unfortunately its chairman, Mr. C. O. Tuckey, on whom much of the work necessarily fell, is abroad on holiday. I very much regret that he is not here to open the discussion, as he would have been the natural person to do it.

But it is chiefly for those who were *not* concerned in drawing up the *Report* that the discussion has been arranged. For that reason I shall confine myself mainly to drawing attention to the salient points of the *Report*.

In connection with § 1, p. 5, it will be interesting if speakers will state their experience with regard to time available, so that we can judge whether the earlier start contemplated in § 7·01, p. 31, is often found possible.

§§ 4, 5, 6, pp. 13 to 30, constitute the main body of the *Report*. Some of the chief points here are the recommendations:

That "force" should first be dealt with in statical rather than in dynamical problems;

that the "law of the lever" is a good starting point;

that the "law of the resolved part" ($P\cos\theta$) is an improvement for beginners on the parallelogram or triangle of forces; and

that forces should not be divorced from the bodies on which they act. (Every teacher of mechanics must recognise how often he has to put to the beginner the question, "On what body are you considering the forces?" It is thought to be a help in this matter to postpone the theoretical type of question about equivalent systems of forces.)

It was § 6 which gave the sub-committee most trouble, and here is an opportunity for discussion. The section is summarised on p. 20.

(·6) would have been left out by some members of the Committee; it represents the views of what may be called the "absolute" school as opposed to the "gravitational". Members of the gravitational school do not want to do without absolute units, but they contend that for the average boy the introduction suggested in (·3), p. 23, is definitely better than the classical procedure through Newton's Laws of Motion. (·5) makes clear the kind of transition that is intended.

This is the place to refer to the controversy on the word "pound". The *Report* takes the view that it will inevitably be used in two senses. The other view is stated by Prof. Sommerville on p. 279 of the current *Gazette*, and there

* A discussion of the *Report* at the Annual Meeting, 6th January, 1931.

is a reply, on the same page, by Mr. Tuckey to which I would like to add one or two comments. Actually Prof. Sommerville's letter has reference not only to the word "pound" (§ 6·2, p. 21, of the *Report*) but to other matters, some of which are dealt with in § 7·05, pp. 41, 42, 43. The idea of insisting on lbwt., *not* lb.wt., on lbl. for poundal, and on ft. lbwt or ft.-lb for ft.lb, seems so near to being a fad that I think the sub-committee must have rejected it even if the matter had come up for consideration. Prof. Sommerville goes, I believe, too far with the Stroud system enthusiasts when he says "g should be used invariably for an acceleration". It is perfectly easy to say "g is the number of units of acceleration due to gravity". Why should that lead to any confusion? The line taken in the *Report* (p. 41) is that in an equation like $P=mf$ the letters stand for numbers. That would apply to $W=mg$. I regard this as absolutely essential in the teaching of young boys, and I would also disagree with Prof. Sommerville when he says that lbl. *is* lb. × ft. × sec.$^{-2}$, because lb. × ft. × sec.$^{-2}$ means nothing. I would prefer to say "g should *never* be an acceleration", because if it is, then, *e.g.* $v^2/2g$, is meaningless. How is it that I can disagree so completely with Prof. Sommerville? Doubtless because we look at the matter from different points of view. I contemplate, and I think the *Report* contemplates, mechanics being taught to quite immature pupils.

In the Stroud system a set of conventions is introduced by which it is possible to assign meanings to things like ft÷sec, or 3 feet÷2 seconds, or v^2/g, where v is a velocity and g is an acceleration. And this is by no means ignored in the *Report* (see the foot of p. 42 and the middle of p. 43).

One cannot assume, however, when one *begins* to teach mechanics to quite young boys that this kind of convention is familiar to them. To begin with lbl = lb × ft × sec^{-2} for such pupils would be like beginning, at the nursery stage, with area = length × breadth. Nor, I think, ought one to be in any hurry to introduce conventions of this kind. The better sort of pupil will almost do it for himself a little later, and the worse sort will fail to understand it if it is forced upon him too soon.

As I have disagreed so heartily with Prof. Sommerville's letter, I would like to express my agreement with the last paragraph in it. "The H.P. of an engine is 7" seems to me to be a mistake like "the price of the article in shillings is 5", but I am not sure that everyone agrees that that *is* a mistake.

Perhaps I should also apologise for attempting to argue with one who, on account of distance, cannot very well reply, but I expect there will be at least one Stroud fanatic present ready to defend the position.

To deal finally and briefly with the miscellaneous sections which are grouped as § 7, ·01, ·02, ... ·15:

·02 should be read in connection with § 3. Some of you may think that we have left out the best experiments; it should be remembered that we assume that only simple apparatus is available. In one way, the simpler the apparatus the better; to illustrate the principles, the accuracy of a Physics laboratory is not required.

·12 is an attempt to make clear why the argument in the inset at the foot of p. 66 is right while that in the inset on p. 67 is wrong; possibly some of you will say that there is nothing to explain; others, that there is a fundamental difficulty which we have failed to explain.

·10, ·13 contain suggestions for dealing with Rotation.

The other sections deal with various points of teaching; some of these may be small points, but most of them are, I think, important ones. It is perhaps a feature of this *Report*, contrasted with the one it replaces, that it deals more with the actual details of teaching. That is why it is longer.

Before I leave the discussion to others, I ought to say that other members of the sub-committee are not pledged to agree with all my remarks. May I also suggest that it will be convenient if speakers refer both to *sections* and to pages if they have occasion to quote the *Report*?

Miss Swirles, in commenting on what she termed the rather controversial section 6, "The Beginning of Dynamics", said she was not so much concerned with the actual beginning, *i.e.* the average class for the School Certificate, as with the Higher School Certificate stage or the very intelligent class for the School Certificate. The *Report* made two recommendations, one by the so-called "gravitational" school and the other by the "absolute" school. While actually more in favour of an "absolute" treatment—certainly at a later stage—the speaker could not support that described in the *Report*. Surely the principle of an "absolute" treatment was that gravity provided many important examples in dynamics, but was not essential; it was not the only force that had to be dealt with, in that almost the whole of the principles of dynamics and development from them were independent of gravity. In a strict development of the subject, the fundamental concepts, definitions and laws should, therefore, be constructed without reference to gravity, however much use was made of gravity in constructing illustrative examples. In the speaker's opinion the "absolute" theory outlined in the *Report* did not conform to the above principle, as the ratios of masses were defined by weighing in a balance—thus introducing gravity from the beginning in a fundamental definition. However, a genuine "absolute" treatment was too complicated to give to the beginner, or even to the Higher School Certificate pupil, straight away; hence the different approach of the "gravitational" school which the speaker endorsed in outline, so long as the idea of mass was introduced before going into the principles of energy and momentum. In the first place, with regard to the fundamental equation $P/W=f/g$, it seemed that in the discussion of the equation in § 6·3 and § 6·4 Russell's principle of writing "df". after definition might have been an improvement. Was $P/W=f/g$ to be taken as a definition or a law? And what was the position of paragraphs (*a*), (*b*) and (*c*), each commencing with "We assume"? Personally, Miss Swirles preferred to take $P/W=f/g$ as a *definition* of ratio of measures of forces applied to same body, though she thought, whichever one did, difficulties arose over comparing forces applied to *different* bodies. That involved, later, the law of action and reaction, which seemed to the speaker rather seriously overlooked in that section of the *Report*. Perhaps that was not of so much importance so far as schools were concerned, but it seemed that it should have been mentioned from the point of view of logical development and treated more thoroughly. It was difficult to discuss the law of action and reaction from the gravitational standpoint in view of the position taken up in the *Report* without bringing in the concept of mass. Actually it seemed that mass would keep coming in, like King Charles's head. Then, the speaker had some doubt as to what was the best way of defining equal forces acting on different bodies. Again, if statics had been started first, the statical definition of ratio of measure of force acting on the *same* body must be linked up with the definition. Personally, Miss Swirles was in favour of putting dynamics first, though she was aware that was not the historical order.

Returning to the question of conservation of energy and momentum, the speaker thought that to write the kinetic energy as $\frac{1}{2}Wv^2/g$, or momentum as Wv/g, was to introduce irrelevant quantities and to obscure essentials. Surely mass was more fundamental than weight, and in using conservation of momentum one could compare masses without any reference to gravity. In passing, the speaker called attention to p. 24, on which it was stated: "Although (ii) and (iii) apply only when the force P is constant, it is important to realise that the interpretations of them have universal application". What exactly did that mean as it stood? It was important, because in the first year at the university one saw students writing $Ps=W(v^2-u^2)/2g$ even when P was not constant.

Detailing one or two smaller points, the speaker remarked that Mr. Robson had stated that the "law of the lever" should be taken before the "law of the resolved part"; simply from the point of view of facility in teaching and

not because of the logical order. With that the speaker had no quarrel, although she thought the law of the lever much more less simple in principle really, and dependent on statics of a rigid body. She welcomed very much the statement on p. 39 of the *Report* that "Teachers of beginners should bear in mind that to speak of a body having two or more velocities simultaneously is to the boy an artificial phrase, perhaps . . . a mathematical fiction". Nearly all text-books were muddled on that point. What one actually had was velocity relative to a frame of reference which also had a velocity relative to another frame of reference, but it was difficult for the common-sense person to conceive of a body having two velocities at once.

In conclusion, the speaker said she particularly wanted information on the position of the Committee in regard to the law of action and reaction.

Mr. **T. C. J. Elliott** endorsed the view that the *Report* was a little conservative, and failed to understand how the teaching differed from that given by his old headmaster, Mr. Sanderson of Oundle, forty years ago. He wondered whether the Teaching Committees always gave sufficient attention to what appeared in the *Mathematical Gazette*. He had attended six or seven meetings of the Association and had never yet had the good fortune to meet any member of the Association who read the *Gazette*. Even distinguished mathematicians who lectured before the Association did not always seem to think it necessary to inform themselves as to what had already been produced on the subject in the *Gazette*. In reference to what Mr. Robson had said about "fads", there was only one point to which time permitted him to refer, and that was the common use of the word "sum" of vectors for what was a clear case of multiplication. From time to time writers called attention to this difficulty, which was especially felt by those who wished to make the study of the dependence and independence of variables the basis of school mathematics (*v. Gazette*, Dec. 1929). The Association had perhaps a tendency to pass by on the other side whenever difficulties of this kind were mentioned.

Mr. **A. N. Fitzgerald**, at the risk of being charged with fanaticism, referred back to the *Report on the Teaching of Mechanics in Schools* issued about ten or twelve years ago, and drawn up by a very strong committee consisting of Dr. Percy Nunn, the late Mr. C. S. Jackson, Mr. David Mair, Mr. A. W. Siddons, and one of the members of the present Committee, Mr. W. J. Dobbs, with Mr. G. Goodwill as secretary. That earlier committee had, before commencing its meetings, sent out a questionnaire to English, Scotch and Welsh schools. Numerous meetings were held, and before it was adopted by the Teaching Committee the *Report* was formally submitted to Professor A. N. Whitehead, who described it as masterly and refused either to add to or detract from that verdict. One would have thought, with such a history, there would have been sufficient light on general and pedagogical principles to last for about a hundred years. It was really a little disconcerting to find that the present *Report* was to replace the earlier one. In the speaker's opinion the Teaching Committee would have rendered better service by reprinting the previous *Report*, and supplementing it by the very valuable details and teaching methods which they had collected. Of course he recognised the amount of hard work that had been put in, especially by the Chairman and Secretary of the Committee; at the same time, he appealed to the Teaching Committee to see that the previous *Report*, which contained treasures of wisdom and knowledge, was not consigned to the waste-paper basket.

Dr. **W. F. Sheppard** confessed that he only spoke on behalf of a small minority, whose views might be expressed by an alteration in a sentence on p. 41 of the *Report*. The sentence is: "It is a commonplace that the letters that occur in a formula of algebra stand for numbers". He would prefer to say: "It is unfortunately true that the letters that occur in a formula of algebra nearly always stand for numbers". His point was that in dealing with a quantity, such as time or distance, we should try to think of the symbol for

the quantity as representing its magnitude, not merely as representing its numerical expression in terms of a particular unit. The sub-committee then went on to say that, in the mechanics formula $P=mf$, m is the number of grams in the mass. When he was at school, fifty or more years ago, m was regarded as the symbol for mass, without reference to any particular unit of measurement; and he could not see that this was unsound.

Mr. Robson had said, in effect, that it was not possible to divide feet by seconds, and had taken objection to the symbol x ft./y sec. But this symbol did not represent the result of dividing x feet by y seconds, any more than $\frac{3}{4}$ meant the result of dividing 3 by 4. The symbol "$\frac{3}{4}$" was primarily only part of the symbol "$\frac{3}{4}$ of"; and "$\frac{3}{4}$ of 24" meant that we divided 24 by 4 and multiplied the result by 3. The symbol to which Mr. Robson took objection had an analogous meaning.

Mr. **W. C. Fletcher** said he was the only other member of the sub-committee present, and therefore he wished to add one or two general remarks to what Mr. Robson had already said. The Committee had been hauled over the coals, quite naturally, as they expected to be, for disregarding the previous *Report*. He thought it fair to say that they regarded themselves as a body of extremely commonplace people unable to walk in the footsteps of their distinguished predecessors, and that the best thing they could do, as the job of drawing up the *Report* had been put upon their shoulders without their request, was not to try to improve upon the work previously done. He thought he was speaking the truth when he said the Committee quite deliberately set that aside. Knowing they could not do anything approximately as good, they were not going to spoil what was already done. Their task was a much humbler one. Incidentally, he might say that it was one thing when full of a subject and anxious to write about it to produce something; it was quite another when, not particularly interested, one was commanded by superior authority to produce something. That was the Committee's apology—he admitted apology was needed. He was speaking quite seriously. The Committee were really in a very difficult position. The best they could do was to try and say what few commonplace things they could which they thought would be of use to ordinary teachers. That brought him to the comments made by Miss Swirles, whose point of view it was easy to recognise. But what position were the Committee in there? He found it rather difficult in following Miss Swirles' remarks to be quite clear as to which of the two schools she was attacking. He thought she dealt out fairly even-handed justice to both. It seemed that the representatives of the "absolute" school were wrong and that the representatives of the "gravitational" school were also wrong; which the worse, he could not be sure. The Committee was not unanimous; that was written on the *Report*. There were two different lines of thought on which, though they got a little nearer together as matters were discussed *ad nauseam*, it had, nevertheless, been impossible to reach solid agreement. That was inevitable. It was impossible to touch the technique of dynamics without running across disagreements. There was one specific point raised by Miss Swirles on which he wished to touch. Referring to par. (·6), pp. 28 and 29, Miss Swirles said the representatives of the "absolute" school were wrong for many reasons, but chiefly because they used the idea of gravity, and she implied that it was quite wrong to make any reference to the balance.

Miss **Swirles:** I may have seemed to be rather carping. I did not mean it in that spirit at all. I speak with the utmost diffidence about teaching in school, because my experience amounts to one week, part-time! I was more concerned with the person going on to the university.

Mr. **Fletcher** added: With regard to the question of the pound. What was a pound? A lump of matter. Some used to say—he thought Maxwell was one—that it was a quantity of matter; at all events that was a phrase that had been in use. Of course it was unsatisfactory. What was meant by

quantity of matter? At once one was pulled up. It was not possible to give a satisfactory definition of mass from that point of view. But the historical point of view was important there. People knew what they meant when they said that two quantities of butter or coal were equal long before they ever thought about gravity. Were they thinking of gravity when they weighed their silver in balances? Before there was any science, people knew, as an experimental fact, that there was a very good way of comparing things by the use of the balance. The point that the *Report* attempted to make in that connection was that this was exactly the convenient sort of beginning. Of course the Committee was aware of the fact that that could not be a final definition of mass. The only satisfactory definition of mass in the "absolute" sense was, he took it, that it was a coefficient in an equation, or something like that. But a beginning could be made with this crude idea of equality of mass, an idea that the ordinary boy or man could grasp and understand and which, if the student went on with the subject properly, would become clarified—certainly by the time he got to the university. If the charge was that teachers did not do their work properly, and did not by the time they sent pupils to the university enable them to understand the subject, well and good, the charge would be right. What it actually came to was that some wanted to begin on absolute values, but could not begin by pure absolute values; they had to adapt themselves to circumstances, and the fundamental point was what was meant by a pound. And then what was meant by two pounds. Thus the *Report* said the test of equality of mass was the use of the balance. Historically, that went back far beyond any idea of forces. In conclusion, the speaker said there was much in the *Report* that he would be sorry to have to defend, and probably the same could be said by his colleagues. They had all done the best they could. They felt somewhat apologetic, not about the necessarily commonplace nature of the *Report* because that was obvious from the outset, but for the rather imperfect, incomplete and incidental way in which matters were treated. When the Committee found something that interested them, something they had to discuss and on which they did not see eye to eye, they thought it something that needed clearing up. That applied to the paragraph in regard to impact and collision which gave no end of trouble. He did not regard it as satisfactory even now, but if members could have seen it in some of its phases they would have thought the Committee had gone off their heads.

Mr. **K. S. Snell** (Harrow School), referring to § 7·12, p. 66, of the *Report*, said it was not clear to him whether the force P acting through impact was a constant or variable force. He did not think the distinction was brought out. The explanation of conservation of momentum was given without any mention of the fact that force was essentially a variable force, and that P must there stand for time average of the force. In discussing the question of loss of energy on p. 68 that question was again of importance. The work done on the rapidly moving trolley was given as $P(a+s)$, while that done on the other in creation of kinetic energy was $P(a-s)$, so that there was a loss of $2Ps$. It seemed to the speaker that that problem could only be tackled by means of the integral Pds as suggested at the bottom of p. 67. He thought it essential to go much more carefully into the integral and definitely assume P as a variable force. Actually in the elementary stage it would be better to appeal to boys' experience to show loss of energy. The question should not be discussed as suggested on p. 68; that stage was surely for the specialist only. The speaker then went on to comment on the section on Experiments on p. 32, and said that he found when teaching boys of fourteen, that they had some difficulty in realising that the tension of string was constant in a pulley system, and also the fact (which the speaker believed was mentioned in the *Report*) that if a pulley was supported by two vertical strings, then the force acting upwards on the pulley was twice T where T was the tension of the

string. Simple experiments with pulleys were, in the speaker's opinion, very easy, and he had found them most valuable. Boys fixed up pulley systems for themselves and convinced themselves on these points. Pulleys were not mentioned in the section on Experiments, but they constituted one of the easiest and perhaps most valuable experiments.

Mr. **R. C. Fawdry** (Clifton College, Bristol) thought the *Report* very sane and obviously drawn up by those who were confronted by the task of teaching mechanics to rather stupid people. The doctrinaire criticisms which came from university teachers were quite beside the point. The sort of criticism that $\frac{3}{4}$ did not mean $3 \div 4$ was the sort of thing teachers appreciated but it was perfectly useless. If they were going to turn $\frac{3}{4}$ into a decimal, were they not going to put down 3 and divide it by 4 ? Criticisms of that sort indicated that the critic did not know what the school teacher's problem was. The *Report* was essentially sensible from the teacher's point of view. The one thing he was anxious about was the suggestion made about the " law of the resolved part " ($P \cos \theta$). There was something dangerous about that. One of the most striking things in teaching experience was that first impressions given to pupils were those that stuck. There was, for instance, trouble in getting pupils to deal with questions from the point of view of energy, because it was more or less necessary to introduce the acceleration equation first and deduce energy from it, with the result that pupils always went to the acceleration equation in preference to the equation of energy. If there was to be talk of the resolved part ($P \cos \theta$) it seemed to him that it would be likely to give rise to a great deal of trouble caused by leaving out the other component. Emphasis on $P \cos \theta$ would later on lead to much labour in instilling into pupils that there was also $P \sin \theta$.

Mr. **S. Inman** (County Secondary School, Isleworth) said there seemed a great deal of trouble over the question of ft. $\div$ secs. Did the critics object to ft. $\div$ ft. ? Would they consider 3 ft. : 4 ft. too difficult for pupils to grasp ? Surely that was a ratio used in most elementary work. If they wanted to use the ratio 3 ft. : 4 ft. would they object to using the fraction 3 ft./4 ft.? He thought nearly everyone would justify use of the fraction and say that it stood for a certain meaning. Shillings could be used in the fractional form to stand for a ratio. For instance, $\frac{\text{3s. 4d.}}{\text{£1 6s. 2d.}}$. He did not think there should be much objection to using ft./sec. on the same lines. As to whether the " gravitational " or " absolute " system should be used, the gravitational system certainly made things very simple for beginners ; but, on the other hand, one ought to recognise that it introduced certain rather serious disadvantages, one of the chief being that it did not give a good opportunity for discussing mass. When mentioning Newton's law of motion there was really excellent opportunity, in discussing the second law of motion, to discuss mass and instil a good idea of what was meant. Personally he thought it necessary to use both systems ; in illustrating a problem one should deal with both methods simultaneously, at the same time drawing attention to the subject of units. It would be a very valuable exercise. In his opinion the *Report* did not deal adequately with the question of impulse and momentum ; it made practically no reference to the principle of conservation of energy as applied to more than one direction. That was a blemish on an otherwise excellent *Report*. In most elementary text-books the principle of conservation of energy was dealt with, and nearly always the only reference was to impact of motions in one straight line. Surely other directions should be dealt with.

The **President,** referring to the last speaker's query as to whether there could be division of 3 ft. by 4 ft., said that, even if it could be done, it was by no means certain what was the answer. Sometimes the answer was $\frac{3}{4}$; sometimes it was $\frac{3}{4}$ of a radian. That seemed to bring out the inconsistency of treating the symbols as not mere numbers but having something which was

rather mysterious attached to them. The method of dimensions was a useful and extremely important principle in mechanics and physics, but it was not a universal method. It was all right up to a point, but an illogicality came in in treating an angle as though it was a number when it was not. With regard to units, the speaker agreed entirely with his friend Mr. Robson and disagreed with his friend Dr. Sheppard, which was somewhat curious because he thought of the question in terms of the tensor calculus and he knew Dr. Sheppard was as enthusiastic about that as he was. But when one got to the general point of view, one found that the dimensions were really a particular case of covariance, and change of units was a particular case of tensor transformation. In his view the fundamental step in mathematical physics was one of replacement of the physical things by numbers. The first step was to replace the entities of mechanics by numbers before the formulae came in. He had been interested to find that Mr. Robson recommended that view not merely for the most advanced but also for the most elementary students of the subject.

Mr. A. Robson did not feel that he could answer all the criticisms, but, dealing first with Miss Swirles' comments, he said that the matter of action and reaction was certainly one which would have to be taken into very serious consideration in the unlikely event of a second edition of the *Report* being required. He could not say more than that on that point at the moment. As to mass, he thought the *Report* did, on the whole, take the view that the idea of mass was one which had to be introduced very early, though it could not be dealt with completely at the beginning and must be taken up again later. He regarded Miss Swirles' criticism of equations (ii) and (iii) on p. 24 as just. The explanation was that the sub-committee was rather unwilling to do anything that was not very elementary and left the *Ps* in some places where they ought to have put $\int Pds$. It was certainly proved on another page of the *Report* (p. 67) that the Committee were aware that $\int Pds$ was more accurate! That remark applied also to Mr. Snell's comments. As to § 7·12, Mr. Robson said he did not fully agree with the explanation as given in the *Report*, so he did not think he should attempt to answer the question. As regards the republication of the old *Report*, that would be a matter that the General Teaching Committee might consider at some future time. The President had answered Mr. Sheppard to a certain extent, and all he (the speaker) wished to add was that he had not intended to demand a meaning for (1 foot)÷(1 second). He had perhaps been rather too severe on the Stroud system in that he had given it as his view that the system would not be satisfactory until a meaning had been given to "feet divided by seconds". What was wanted was not a meaning *in isolation* for "feet divided by seconds"; it was a meaning *in use* which came to much the same thing as Mr. Sheppard's $\frac{3}{4}$ of 24. But he disagreed with Mr. Sheppard's view as to the meaning of $\frac{3}{4}$, although he did not imagine that $\frac{3}{4}$ meant $3 \div 4$. Mr. Robson felt doubtful as to the point raised by Mr. Fawdry in connection with $P \cos \theta$. There was, he agreed, danger of emphasising that too much. The idea was that for the introductory lesson in resolving force one should think of something in a groove being pulled along by a force P at an angle θ, and that one should ask what was the effective force along the groove. That was intended as the thing to do at the elementary stage. Mr. Inman had contended that both the "absolute" and "gravitational" system should be used together. He thought that was also the intention of the sub-committee. It had been stated, in the *Report*, that even those who had gone over to the "absolute" system and used it habitually would sometimes go back to the gravitational system in dealing with appropriate questions.

The President: We have had a most interesting discussion on the *Report*, and I am sure that the members of the Association very much appreciate the labour that has been spent on preparing the *Report*. Everyone, while disagreeing with portions of it, will find matter for use and stimulation.

GAMBLING.*

Mr. W. Hope-Jones: The subject of gambling is one that needs to be discussed by many different kinds of associations, each in its own way. Religious and ethical bodies would do well to give more attention to its moral effects and to the reasons for believing it to be wrong, or believing it to be right, or for believing it to be wrong in some cases and right in others. Societies of lawyers ought to debate the present chaos of the betting law. All athletic clubs ought to be taking counsel how to preserve the honest sports which still survive from being swallowed up by the gambling interest. Finally, the contention that games are not interesting enough to play for their own sake, without money having to change hands on them, is one which I commend for study to the Association for the Care of the Mentally Defective.

In this Association, clearly a treatment is required which must be largely mathematical; and you will expect me to begin with a definition. But instead I will offer you that corner-stone of wisdom known (to me) as Hope-Jones's Axiom, which is: "No definition of anything can satisfy the professional philosopher and also mean anything to the ordinary bloke" (to which it may be added that jolly few do either). But because in my profession I am mainly concerned with the ordinary bloke, and particularly with myself, I make no pretence of offering you a philosopher-tight definition.

Peter Green, Canon of Manchester, defines gambling as "an agreement between two parties whereby the transfer of something of value from one to the other is made dependent on an uncertain event in such a way that the gain of one party is balanced by the loss of another". This definition breaks down because it includes insurance, which is clearly an agreement between two parties whereby the transfer of something of value from one to the other is made dependent on an uncertain event, in such a way that the gain of one party is balanced by the loss of another. Yet we know that insurance is in principle the exact opposite of gambling. (I shall say no more about insurance, because one of the later speakers intends to deal with that.) Green also quotes Professor Sorley's definition: "to engage in a transaction involving risk and uncertainty, in which the gain to one party is balanced by the loss to another party". Green very justly criticises this definition as including the activities of the pickpocket, who engages in a transaction involving risk (because he may be caught) and uncertainty (because he doesn't know how much my purse contains), and his gain is exactly balanced by my loss. (I assume here that both Sorley and Green are using the word "gain" to mean material gain, and not including moral gain in their definition.)

Five years ago I was fortunate enough to inherit a copy of Whitworth's *Choice and Chance*, a literally priceless book now that no money can buy it. It is really discreditable to British Mathematics that this great work has been allowed to go out of print. In it Whitworth defines gambling as "the act of exchanging something small and certain for something large and uncertain". This, of course, would include divorcing your present wife in the hope of marrying a fatter one of uncertain age and temper.

Now the fact is that all of us, even those who disapprove strongly of gambling in material possessions, gamble freely in most other things. Happiness, for instance. Anyone of the sort that we aim at being would readily barter away the certainty of a contented animal life, warm, well-fed, untroubled and unambitious, for the chance of happiness of a larger kind, such as wearing yourself out over a job you believe in, getting burnt at the stake for a great cause or drowned in rescuing a child, discovering the Kingdom of Heaven or understanding Einstein. Then consider time. I confess myself an extremely far-gone but quite unrepentant gambler in time. Many golden hours which

* Discussion at Annual Meeting, 6th January, 1931.

might be soundly invested in reading the accepted wisdom of ages, in conventional amusements, or most of all in sleep, I willingly spend in such gambles as listening to sermons, trying problems which are mostly too difficult for me, making acquaintances who frequently let me down badly, or working up a subject for meetings like this. It is a lottery, in which the second prizes are ideas and the first prizes are friends. But the alternative to gambling in time is to miss the pearl of great price by being too cautious to buy acres and acres of worthless field along with it. We know the kind of mathematician who has mugged up all his bookwork but never barked his shins on the rocky paths of original investigation. He gets marks in an examination; but are these the men who make new worlds? What gamblers Moses and Columbus were! And where we find even greater characters than Moses and Columbus, I suggest to you that they were even more incurable gamblers.

Shall we then take the gambling with which mathematicians are concerned as being "the voluntary exchange of some smaller but certain material value, whether positive or negative, for a greater but uncertain material value"? If there is a difference in principle between gambling with material property such as money and gambling with immaterial things like time and happiness, I think it lies chiefly in the fact that when the gambler wins money he wins it by making somebody else poorer, while in other ways we may enrich ourselves without impoverishing our neighbour. I believe that is the reason why, though responsibility for ownership and responsibility for other kinds of power and opportunity are equally real, yet they differ in their nature: whether that is so or not, wherever we find a high sense of responsibility for ownership we find with it an aversion to making ownership depend unnecessarily on the spin of a coin or the turn of a card.

One's first instinct is to expect material values like money to be more amenable to mathematical treatment than invisible things like time and happiness. But I have not found that this is always so. Take, for instance, the obvious fact that the value of money is not proportional to its quantity. If I waste 200 hours of your time collectively by talking rubbish for two hours to 100 of you, I have done five times as much harm as I would have done if the audience were $\frac{1}{5}$ as big or the oration $\frac{1}{5}$ as long. But if I give sixpence each to the entire population of this planet, I have not benefited them collectively six times as much as if I give them a penny each. And none of us would consider a gift of two million pounds twenty times as desirable as a gift of £100,000. In fact, I would distinctly prefer the £100,000, as being enough to bring me at least as many opportunities as I could use wisely.

When we come to the mathematical concept called "expectation", it becomes even more glaringly obvious that the human value of an expectation differs widely from the sum of money multiplied by the fraction representing your chance of getting it. Even those of you who would rather have £2,000,000 than £100,000 would very much prefer a certain £100,000 to $\frac{1}{20}$ chance of £2,000,000, or even to a half chance of £20,000,000, whatever its mathematical expectation may be. In fact man shall not live by expectation alone. Or coming to negative values, most of us would rather have $\frac{1}{5}$ of our income taken away from us in taxation than face $\frac{1}{5}$ or even $\frac{1}{20}$ chance of losing it all.

A few exceptional cases may be found, mostly rather far-fetched, in which the human value of an expectation is greater than its mathematical expression. Suppose that A's wife urgently needs an operation which costs £200, that nobody will lend him money, and that the utmost he can raise is £180. In that case a $\frac{9}{10}$ chance of £200 and a living wife is worth more to A than a certain £180 to bury her with; and I for one would not think him wrong in gambling on a system designed to win him another £20 at a small risk of losing all or nearly all his capital. But that is not the kind of gambling which we find actually prevalent in the world we live in. In nearly every case the real value to a human being of an expectation of money is less than its mathematical

expression; and in nearly every case the gambler pays more than this mathematical expression as a price for it.

Another instance of a gamble which seems to me justifiable. The Duchess of Balham and Tooting dies and leaves Saint Jehoshaphat's Hospital her celebrated painting of "The Space-time Continuum" by that master of Old Masters, Gorgonzola, valued at £20,000. Owing to trade depression nobody is ready to offer more than £1000 for it, and art experts are unanimous in the opinion that to cut it up and sell it by the square inch would seriously impair its value. (Or in view of the subject, should it be sold by the cubic inch?) This seems to me the sort of case in which a raffle is the right way to dispose of it. If a thousand people can give Saint Jehoshaphat's Hospital £20 each without neglecting some important duty, and if they can be induced to do so by offering them $\frac{1}{1000}$ chance of possessing Gorgonzola's immortal masterpiece, and if furthermore this is the only available way of disposing of it for its real value, then I see no mathematical or economic objection to such a raffle, which is the nearest practical equivalent to selling the picture in little bits without spoiling it, while obviously in principle it differs radically from the Irish sweepstake with its money prizes, or the raffles where motor-cars are bought with the express object of raffling them. In fact such emergency methods of meeting a real human need ought not to give more offence to the anti-gambler than a doctor's prescription of a medicine containing alcohol gives to the ordinary abstainer.

Of all the material arguments against gambling, perhaps the most cogent is its effect on the distribution of wealth. Not being all Communists, we are not agreed that all wealth should be divided equally between the inhabitants of our planet. Not being all Socialists, perhaps we are not even agreed that a redistribution of wealth, more in proportion to needs and services, is urgently called for. But, I take it, we are all agreed that wealth in this world we live in is much too unequally distributed. "By this time", says Bernard Shaw, "nobody who knows the figures of the distribution defends them. The most bigoted British Conservative hesitates to say that his king should be much poorer than Mr. Rockefeller, or to proclaim the moral superiority of prostitution to needlework on the ground that it pays better"—after which he comments caustically on the gross disparity between a bookmaker's income and an astronomer's. I have never heard any economist suggest that the cure for our present ills lies in taking money away from the poor to give it to the rich. Yet this is the principal effect of gambling as carried on in the world to-day.

Start with a simple case. A has £8, B has £3. They toss a coin repeatedly for a £1 stake. Find the probability that B loses his £3 before relieving A of his £8.

Let $f(x)$ = the chance of the whole £11 passing ultimately to a man who has £x when the other has £$(11-x)$.

Then after the next toss this player has either £$(x-1)$ or £$(x+1)$, and the chance of each of these is $\frac{1}{2}$; thus

$$f(x)=\tfrac{1}{2}f(x-1)+\tfrac{1}{2}f(x+1).$$

That is, $f(x-1)$, $f(x)$ and $f(x+1)$ are in Arithmetical Progression; hence

$$f(0),\ f(1),\ f(2)\ldots f(10),\ f(11)$$

are in A.P. But $f(0)=0$, and $f(11)=1$; whence

$$f(3)=\tfrac{3}{11},\ \text{and}\ f(8)=\tfrac{8}{11}.$$

Because the sum of these $=1$, either A or B must be ruined if they go on long enough, and the odds are 8 to 3 that it will be B.

If this were intended for a complete demonstration of the principle that gambling enriches the rich by ruining the poor, a few seconds' thought would reveal several flaws in its claim; but as far as it goes it is significant of a real tendency. "To the community", says Whitworth, "gambling is disadvan-

tageous because its tendency is opposed to the equable distribution of wealth. It tends to accumulate property in a few hands, making the rich richer and the poor poorer. This tendency speculation introduces into trade."

The great continental casinos and state lotteries are rich institutions which enrich themselves at the expense of those who, on the average, are poorer. So are our English bookmakers.

Here are some figures from *The Times* giving the odds against fifteen horses running in the "Tattenham Plate" race at Epsom:

Against	WEYHILL	6 to 4
	MORVINA	9 to 2
	BURSAR	8 to 1
	LAMPLUGH, LADY JOSEPHINE	10 to 1
	CARAMELLA	100 to 8
	GALLANT JACK, W. AND G., SORRENTO, TEO, DESMOND LASSIE, Filly by KILLEAGH—MORAL MARY, LAIDIPIA, PRINCESS MABEL, FAIRY	100 to 7

To make sure of winning money, the bookmaker needs no knowledge whatever of horses: he only needs to arrange his bets so that on every horse the total sum at stake is approximately the same.

Name of Horse.	Bookmaker loses if horse wins. £	Bookmaker wins if horse loses. £
WEYHILL	66	44
MORVINA	90	20
BURSAR	96	12
LAMPLUGH, LADY JOSEPHINE	100 each	10 each
CARAMELLA	100	8
9 others	100 each	7 each
		£167

By adding in the last column we find that before the race the bookmaker receives £167 in stakes. After the race he pays out

£110 if Weyhill, Morvina, Lamplugh or Lady Josephine wins;
£108 if Bursar or Caramella wins;
£107 if one of the nine others wins.

So the bookmaker's profit is £57, £59 or £60.

In practice it isn't possible for him to keep so near to equality as this; but he keeps as close to it as he can by raising his prices against any horse about which he has too many bets, and lowering it where he wants to attract more. (Lady Josephine won this race.)

In the one year 1929, the sum spent on betting on horse-races in this country was estimated at £267-million, and this was before the great spread of the totalisator. If we take this "Tattenham Plate" race as typical of bookmakers' odds, about 93 of these 267 millions must have stuck to the bookmakers, enough to keep 32 Mathematical Associations going since the birth of Archimedes till now. In football betting, especially as conducted by newspapers, the odds are hugely more unfair than in horse-racing; and add

to that "the dogs", where 3 per cent. of the time is spent in racing and 97 per cent. in betting.

The economics of speculation on the Stock Exchange are far too large a question for me to go into. There is some similarity between the speculator and the glider. Each rises by work which it has not done itself, by taking advantage of fluctuations in the real driving forces. But the glider is at any rate a thing of beauty, and I have not heard the same said about the speculator. When a speculator gains money, it is a difficult problem to settle who has earned it for him; but it is very obvious who has not. A closer comparison really is between an industrial concern and a rowing-boat, in which the work-people are the oarsmen and the directors and investors who feel their responsibility do the steering. The inert, unthinking shareholder is a passenger, and many feel that the boat would go better without him; while the speculator is the sort of passenger who rocks the boat for fun; and there can be no doubt at all that the right place for him is overboard.

We need to purge our arithmetic books of the questions in which speculation is held up as the *summum bonum* of investment. Take also the questions which begin "Which is the better investment?" Four things make a good investment: first, a cheap price; second, a good rate of interest; third, security; and fourth, that the investor should believe in the work his money is doing. Of course the last matters more than all the other three together; but of the three material factors the third, security, is infinitely the most important. Yet arithmetic books give price and rate per cent. as the only data on which children are to decide "Which is the better investment?" No wonder that their ideas on the meaning and purpose of investment are hideously distorted, some of them regarding the whole Stock Exchange as nothing but a gigantic gambling concern sanctified by the livelihood it affords to their own fathers and uncles.

Finally, we must teach Probability. Those of you who were living seven years ago will remember that I swallowed up a valuable hour of your lives in putting forward from this platform "A Plea for teaching Probability in Schools", which you never acted on. Perhaps you have not another spare hour now to hear it all over again; but here is the last sentence of it for you: "For the slaves of all these superstitions there is only one remedy: 'The truth shall make you free.' And it is our business to consider whether a large part of the mathematician's contribution to that truth doesn't come under the head of Probability".

Dr. **F. J. W. Whipple**: I suspect, as a matter of fact, that of all classes of the community mathematicians are least prone to gambling. A knowledge of the theory of probability is likely to produce a vivid appreciation of the discrepancy between the cost of gambling and the expectation of success. It is, therefore, rather curious that we are in the habit of assuming that the language of the gambler may be used for stating probabilities. We say that the odds are two to one in favour of a fine day to-morrow when we mean that the probability that it will be fine is one in three. As long as no one is actually betting no confusion arises, but in any discussion of gambling the distinction between the terms of bets and estimates of probabilities becomes important.

Consider the simple case of betting on an event like the Boat Race where there are only two competitors. The backers of Cambridge display more enthusiasm than the backers of Oxford, and eventually bets satisfactory to both parties are made at 3 to 2 on Cambridge. I suppose that a supporter of Cambridge has a vague idea that if he could back crews repeatedly under similar circumstances he would make money in the long run, and the supporter of Oxford has the same idea. If that is so, we may say that the odds do represent a rough mean between the estimates which might be made by the two parties of the probability of a win for either crew. The backer trusts that his judgment is better than his antagonist's, but his aim is not so much to

win money from the other man as to get excitement for himself. I feel that that is a side of gambling which does not receive due consideration. What some people are out for most of the time is to get amusement, and by gambling they can get amusement without too much intellectual effort. Games and races vary in their own intrinsic interest, and the more intrinsic interest they have the less necessary the stimulus of gambling becomes. People think it is not necessary, and therefore that it is wrong, to have money on a cricket match, because they probably have sufficient enthusiasm for the teams and for the individual players, but no one expects the man in the street to have any particular enthusiasm for a horse. He will only become an enthusiastic interested party in a horse-race by having money on the race.

To watch a race without caring which competitor is the victor is dull work. To watch a race knowing that the success of one competitor will bring you a bonus is a pleasureable excitement. This excitement is worth paying for as much as any other entertainment. The moralist who argues with the racegoer, pointing out that the bookmaker's profits are evidence of the foolishness of backers, does not appreciate this factor. The bookmaker earns his living as a purveyor of excitement.

I must confess that until recently I made a similar mistake. I have often read the tips given by sporting correspondents in the newspapers and looked for references to the proper odds, but I have never found any. A mathematician discussing the chances of horses before an important race would say that he thought the probability of a victory for "Blanco" was ·20 and the probability of a victory for "Evening Star" was ·05. To make himself clear to his readers he might say that he would recommend backers to accept odds of 5 to 1 against "Blanco" but to refuse to bet if the bookie would not give a better price than 3 to 1. He might say that anyone who could get 20 to 1 against "Evening Star" would have a fair run for his money.

The sporting correspondent, on the other hand, merely says that "Blanco" has done well in the past and is his tip for the race, whilst "Evening Star" has a bad character and cannot be trusted to complete the course. The sporting correspondent is, I think, in the right. He says to his clients: "You want the excitement of seeing the horse which carries your money coming out a winner, or almost a winner. You should back 'Blanco'. If you put your money on 'Evening Star' you will have little excitement. The horse might possibly pull through, but it is most likely he will be beaten early in the race. That will be very dull for you". I do not know if this is quite a fair way of looking at the question. Can racegoers and their advisers be so unsophisticated?

The prices of horses are apparently adjusted without regard to numerical estimates of the probability of their success in a race. The prices are fixed by the law of supply and demand. In so far as a bookmaker lives up to his name and is able to make a book on each race, the prices are an index of the backing given to the different horses. The fact that a victory for the favourite is always regarded as involving the bookmakers in loss shows that a bookmaker does not balance his book on each separate race. He makes his profit on his takings over a long period.

Perhaps the strongest evidence that the backer of horses makes no definite estimate of probability is the extent of starting-price betting, in which the backer is content to stake his money without knowing what the odds will be. I understand that if you find a bookie's runner round the corner and give him a betting slip you will be making a bet on a race, though you will not have the vaguest idea what the odds are at which you are betting. They will be settled miles away on the racecourse. That seems a poor way of proceeding. All the same, to criticise gamblers for not making wise investments would be absurd. We must recognise that they are paying for excitement in order to relieve a dull life.

The totalisator differs from the bookmaker in that it is bound to make a book on every race. The profit made by the proprietors is a fixed percentage of the total amount staked. In practice the followers of the favourite horses receive, if successful, about the same sums of money as they would have gained had they patronised the bookmakers, but those who back an outsider and win are much more highly rewarded. It looks as if a professional backer who put his money on all outsiders with little regard to form would make a handsome profit. Unfortunately, the statistics by which the functional relation between the popularity of a horse and its chance of winning could be determined are not published. I had the curiosity to write to the Betting Control Board to ask if they had any suitable statistics, and they told me if I looked in the *Sportsman* every day I should find the figures I wanted. I invested twopence in a copy of the paper and found that on that day the figures were not there, so I did not become a subscriber. No doubt the extent of the backing of individual horses is given for important races. For a statistical study such information with regard to a large number of races would be required.

It would be legitimate, I think, if the proprietors of the tote varied the percentage of their levy so that the followers of successful outsiders would be less extravagantly rewarded than they are at present. The operation of the tote could be assimilated to the practice of bookmakers who spread their gains and losses over several races. I suppose, however, that that would be rather a jar to public opinion, which welcomes the establishment of the tote because of its greater fairness. From a mathematical point of view, it is not so fair as the system in which each bet is made at definite odds, because one cannot help thinking that the proper thing to bet about is the probability that a horse will win the race rather than the probability that other backers will not be numerous.

Another point to which I wish to refer is the relation of insurance to gambling. It is often said by quite responsible people, like mayors and magistrates, that a man who insures his life is making a bet with the insurance company, and that if gambling is to be condemned then insurance must be included in the indictment. In reality it is the man who does not insure his life who is the gambler. He leaves to chance the future of his dependants. By insuring his life a man makes a contract which is of the nature of a bet. The company offer him long odds: "We bet you 20 to 1 in hundreds that you will not die before this day next year". The company's business is very much like a bookmaker's, for the prices charged to clients are calculated so that, whilst there are heavy losses on some contracts, profits, on the whole, exceed losses. The man who takes out an insurance policy is, however, not in the same position as the bookmaker's client. By insuring, he is avoiding the risks involved in his bet with fate; in other words, he is hedging. This aspect of insurance is, I believe, regarded by the law. It is in general forbidden to take out an insurance policy when there is no insurable risk. To insure a child's life when the death of the child will not involve the parent in direct financial loss is, I understand, illegal. The practice of insurance is designed to reduce risks, not to increase them. Finally it may be noted that whilst racecourse betting is conducted with little regard to numerical estimates of probability, insurance is the principal field of application of the theory of probability. It is to actuaries we turn to learn the latest developments of the theory in its practical application.

Mr. **Hope-Jones** then read the following note sent by Mr. P. M. Marples, who regretted his inability to be present:

Mr. **P. M. Marples**: Gambling appears on our agenda by virtue of its essential ingredient "chance", whose analysis is mathematical. The greater the part played by pure chance in a transaction, the greater the element of gambling. We see gambling at its purest in the pleasant pastimes of "spotting

winners with a pin", purchasing tickets for the Calcutta Sweep, and so on. But skill and experience may count for much in other forms of gambling. The real racing man, for example, in time acquires "horse sense", by which he reduces as much as he can the vagaries of "pure chance". The mathematical machinery of "odds" and betting are merely a convenient calculus for expressing the results of cogitations which have no mathematical basis.

I do not propose to deal with the moral aspect of gambling; I shall be satisfied if I can show that it is—to parody a famous old chestnut—"worse than wicked, it's stupid!"

The real gambler seems to me to be the victim of sundry delusions from which none of us are altogether free. The first of these is "luck". The gambler believes that chance will favour *him*. He is a "hopeless" optimist—defining that person as one who is convinced that if anything unpleasant is going to happen it won't happen to *him*; the pessimist, on the other hand, being one who has been living three weeks with an optimist.

Mr. Hope-Jones has sent me his opening remarks in skeleton form, and I fancy that I see a trace of this delusion in his example of the man who is prepared to risk the whole of his £180 to secure the extra £20 that he needs. This I take it he may do by purchasing nine £20 tickets in a lottery of ten tickets, each carrying an equal chance of a prize of £200. I suspect this good man of harbouring a feeling that because his ultimate purpose is good, therefore fate is certain to be kind to him. But what if the tenth ticket wins? Will he feel it is unfair? I should like to overhear a conversation between the two ticket holders if this event occurs. As an alternative to parting with his £180 for lottery tickets, I should advocate waiting until the draw has taken place and then approaching the winner for a loan of £20.

In fact there are only two possible results—he may win the coveted £200 or he may lose his £180, all he has. It is impossible to foretell which, and I for one would not jeopardise my precious £180, for if I did lose I should be so very much worse off than before.

I disagree too with what I take to be Mr. Hope-Jones's contention that the cash value of a wager may equal or even exceed the mathematical expectation. This expectation is a hybrid of gains and losses combined in a proportion assigned by theory or by previous experiment. A single event results in either total loss or total gain—how, then, can it be valued? A *series* of single events is another matter altogether. A serial ticket for, say, 1000 lotteries may be regarded as having a value, as losses and gains may happen to follow some mathematical law, but the single event cannot be valued.

Another illusion of the gambler is that there is some power controlling blind chance so that the average predicted by mathematical considerations shall be preserved. Let us suppose that a coin has been tossed 10 times and has fallen head on each occasion, and that a wager is suggested on the result of the 11th toss about to be made.

One gambler will say that, as in the long run heads will appear no more frequently than tails, it is high time tails appeared, and he will bet on tails "as the average must be preserved". Another type of mind might say that there is something peculiar about that coin to make it fall head, and will predict yet another head. The canny member of a mathematical association will shake his head and refuse to bet. If we want to test the matter experimentally, we must toss our coin, and, recording only the cases in which ten successive heads appear, note the proportion of heads to tails in the 11th tosses. I shall be surprised if the proportion differs materially from equality. It may interest the Association to know that years ago I set a class of seniors to tossing a penny 100 times, and that one honest fellow returned a total of 85 heads. I calculated at the time that if all the unemployed in Birmingham were set tossing pennies at the rate of once every five seconds, eight hours a

day, six days a week, one might expect a repetition of this event once every three months on the average. I have no wish to spoil sport, and therefore refrain from mentioning the number of unemployed—it can be calculated by the curious.

This conception of a force controlling chance so as to produce a stated average is at the bottom of many a gambler's "system". In fact it is a nice question whether the gambler is not to be defined as a person who does *not* believe in pure chance.

Every person who dabbles in Probability knows the difficulty of the phrase "at random". In any practical experiment it is exceedingly difficult to secure "randomness". To shuffle a pack of cards so that the new order may be independent of the old one takes some doing; I suggest tossing them in the air on a windy day and collecting them again!

I was once asked by Mr. Cary Gilson whether I believed that if a frying-pan were filled with equal quantities of black and white sand grains and then shaken there would eventually appear my name in capital letters. Theoretically, there appears to be no reason why this should not happen, provided one kept on long enough. Practically, however, any fresh arrangement of grains is affected by the previous one so that randomness is unattainable. If the two sorts of sand were originally separated by a diameter, would the grains ever be thoroughly mixed by shaking?

Perhaps, then, there is something to be said for the gambler's disbelief in "pure chance" and the view that previous events do have an influence over subsequent trials.

The mere taking of risks does not make a man a gambler. Many risks in life must be faced—we have no option. To launch out in some new direction in middle life means taking many risks—against which one pits energy and resolution, wits and hard work. The gambler deals with risks which are not affected by any action of his; any skill he may have is directed to selecting that risk which is smallest in itself and is fraught with the greatest advantage to himself.

As Dr. Whipple has dealt with insurance, I will not say much about the difference between this and gambling. I have no sympathy with those who see no difference. Does any one lose in an insurance transaction? One may pay fire premiums for twenty years without having a fire, but one gets in return what the insurance man calls *Cover*, resulting in a peace of mind which is not at all to be ignored by the most materially minded business man. In the early days of life assurance it may have happened that a man paid in more in premiums than would be drawn—I had a long-lived great-uncle who complained that way—but more modern schemes of assurance forbid it. The invested funds bring in a steady return which secures that every assured life wins a "prize".

Professor **E. H. Neville**, speaking from the point of view of the possibility of improbable things happening, said a diary had come under his notice in which among mathematical questions there was one as to odds against a particular distribution in cribbage. The diary was that of the year which contained the question, the answer not being given. There was, however, a pencil note as follows: "The odds against this distribution are £292 15s. 7d. to 1d., and yet the thing did happen to me once in my life".

Mr. **Oldham** (Midsomer Norton): One of the most difficult problems of life is the finding of subjects of conversation; and here, more than is realised, is the chief contribution of gambling to social life. It is so easy to talk of the prize nearly won, of acquaintance with the man who did win, and so on. And in the workshop, though not perhaps in the schoolroom, gambling provides a very large part of the conversation.

The **President** thought one of the simplest illustrations of the fact brought out by Mr. Hope-Jones with regard to the difference between mathematical

and human value was the very well-known example in probability known as the paradox of St. Petersburg. Two men tossed a penny, the bargain being that Peter was to receive 1d. if heads came the first time; 2d. if the penny did not come down heads the first time but the second; 4d. if it did not come down heads the first two times but the third; and so on in geometrical progression. The question was how much ought Peter to pay Paul for the expectation? That was quite simple to work out. There was, of course, half a chance that 1d. would turn up heads the first time, and as the winner would receive 1d. in that case and had half a chance, that would be ½d. If that did not come off he had another chance in that there was a quarter chance of the penny turning up heads the second time. In that case the winner was to receive 2d. One-quarter of 2d. was also a halfpenny; similarly there was an eighth of a chance of receiving 4d.; and so Peter ought to pay an infinite sum for the expectation. Of course all mathematicians knew it would be very foolish to pay half a crown for the expectation. That was a case in which the mathematical was very different from the human value. If carried to infinity it was vitiated by the fact that obviously the winner would pay for a chance of expectation of something which he would not get if he won it. There was another elementary point which it seemed well to recall, namely, the $\sqrt{n}$ law. Assuming in the case of bridge players the average night's win or loss was 1s., then for one hundred nights the average gain or loss would be 10s.; for 10,000 nights it would be 100 shillings, and so on. That was one of the more moral conclusions of mathematics on the subject of gambling. How often players remarked that they were all about equal skill, and so they gained one night and lost another, and thus, on the average, things worked out evenly. There was, of course, a fallacy there, in that the total tended to increase and there was no evening out, and if play continued long enough it was highly probable that a player would either win or lose a fortune. On the other hand, the fact that it was $\sqrt{n}$ and not n introduced what one might call an immoral conclusion of mathematics, namely, that if a man was not playing bridge he would be amusing himself in some other way which would not be entirely costless. Supposing one reckoned the value of the entertainment at any small quantity, b, for instance one penny per night, then the value for n nights' entertainment became nb, and, of course, as n grew large, it exceeded the loss or gain $\pm a\sqrt{n}$. That was the principle on which in roulette it was arranged that the bank always won. The practical consequence seemed to be that if one was really resolved to gamble, the best thing to do was to do the thing thoroughly and stake all on one big chance. If divided up into little chances, knowing that a certain amount disappeared all the time as the tax of the gambling establishment, the amount won in the long run is bound to be less than the tax. Then there had been some reference to the fact that gambling was a subject for discussion by legal societies. It always had seemed to the speaker that the law as to lottery was extraordinary. It was supposed not to be a lottery if there was skill in it. There was, of course, a great deal of skill in spotting a winner in the case of horses, whereas in practically all lotteries, even if a certain amount of skill was needed in guessing certain things, the skill required was almost infinitesimal as compared with that in many of the more recognised forms of gambling. It was extraordinary that the minute element of skill called for in the case of a lottery should be held to make it not a lottery but, in the eyes of the law, a "competition".

Mr. **E. H. Lockwood** regarded the St. Petersburg problem as an excellent example of the misuse of the word "infinity". It sounded extraordinary to say that the expectation was infinite, but it was perfectly right to say that the expectation was greater than any sum one liked to mention. If one offered 2s. 6d. for one's chance of gain one would be perfectly justified and wise provided one could play long enough. To offer £100, one should be prepared to play the game not only for the rest of one's life but for a good many centuries.

There seemed a great difference in the meaning of the phrase if put in the more exact form, "greater than any sum one liked to mention".

Mr. **Katz** thought that despite everything that insurance companies could do and all the safeguards civilisation could devise, there seemed a very large chance factor that it was impossible to get over. What was to be done when there was something in the world for which there was no obvious remedy? The only thing to do was to make a game of it. Something similar happened when human beings were confronted with death, from which there was no escape. Tragedies had been written in which the hero died or came to an unfortunate end, and yet people did, as a matter of fact, derive considerable satisfaction from seeing a tragedy. They were not distressed. On the contrary, they were elevated because they realised how strong human beings were; and they treasured the picture of the battle they put up in order to escape the inescapable death. A danger was converted into an imaginative gain. Similarly with regard to the passion for gambling: people realised that they were subjected to the chanciness of life. Civilisation provided all sorts of ways of dealing with the chanciness of life. Nevertheless when one had exhausted the resources of civilisation, the chanciness of life remained. One of the ways of soothing one's fears was to get the excitement of a game out of them. One wanted, as it were, to enjoy the chanciness of one's own life. He suggested that for Mr. Hope-Jones' consideration.

Mr. Marples had referred to the tossing of a penny which fell head upwards ten times, and speculated as to the chances of the head appearing the eleventh time. A sceptical person might say there was something funny about a penny which turned up heads the eleventh time, but that was only another illustration of the degree to which mathematical calculations were out of touch with ordinary experience. One saw the thing more clearly by taking not ten but twenty tosses. Mathematically, the probability that there would be a run of twenty heads was half of twenty, but obviously if he had seen the penny turn up heads twenty times and was asked what was the probability of heads appearing the twenty-first time, he would say that it was a "dead cert." It would be a question of what was known as initial probability. The initial probability was that there would not be a run of that sort. As all knew, Mr. Keynes in the first hundred pages of his great work on probability had given any number of instances showing that what mathematicians worked out as the probability and what was the probability when applied were quite different questions. When working out a thing mathematically it was necessary to have complete control of all the factors, and to lay down in advance that this and that event were equally probable; but the moment one got to brass tacks and tried to apply in real life what was meant by "equal probability" the difficulties began.

Prof. **J. E. A. Steggall** said that a good deal of the debate had turned upon some humorous coincidences that had arisen and might arise. He would tell them of a curious event in his own experience, and of another event in regard to which his own faith was not complete. As to the first, he was an extremely bad bridge player; but during a Christmas party, five times running he made five diamonds, having declared them to the horror of his partner. He then went down five hundred odd. In each of the next two hands one card was played, and in each he threw his thirteen cards on the table, making a grand slam each time. That happened in a couple of hours' play. It was exceptional, but illustrated the possibility of the President's imaginings. As to the second instance, there was a bridge party in which one person of loquacious character told his table that he had recently played a game in which one person held all the clubs, another all the hearts, another all the diamonds, and another all the spades. Of course no one at social parties would venture to contradict anyone else. But another player then said quietly that he could tell of a still more remarkable thing that happened when he was playing. One player, he

said, had all the clubs, another all the hearts, another all the spades, while the fourth had four aces, four kings, four queens and a knave. Thereupon the first speaker looked at the other and said, "That's not nearly so remarkable". The second person replied simply "Isn't it?" and amidst loud laughter he made for a fortunately handy door.

Mr. **A. Robson** mentioned one small incident which had happened to him only the previous day. He had asked a friend to choose a number, or year, at random. Those who knew anything about prime numbers would know that the chance for a number chosen at random to be a prime number is zero. To his astonishment his friend chose the number 1931! So that an event, the probability of which was zero, had actually happened to him. That was really an illustration of the fact that one could not do a thing at random!

The **President** said the debate had called to his mind an example in probability which he had sometimes been in the habit of setting his class because it brought out the points he was trying to get home: "If A, B, C and D each speak the truth once in three times, and on a certain occasion A affirms that B declares that C denies that D is a liar, find the probability of B's statement being true". It seemed that the time had come to end the discussion and call on Mr. Hope-Jones to reply.

Mr. **Hope-Jones** feared his reply would be somewhat disconnected. Mr. Marples had denied the reality of expectation, he thought, in single cases. Personally he could not define the human value of expectation. He believed that it bore a very real relation to mathematical expectation, being occasionally equal to it. He had, however, already put forward the view that actually people did order their lives very largely by considerations of expectation, human expectation bearing a distinct relation to mathematical expectation. In that case it seemed hardly possible to say that no such thing existed, unless one used the word "exist" in one of the senses philosophers attached to it, in which case nothing existed whatever.

The fact that a person would not be willing to accost the first man he met and offer him a guinea if he could guess the day of the week on which he was born (that is, offer him an expectation the mathematical value of which was 3s.), all went to prove that, because one would not offer it for nothing, an expectation, whether it "existed" or not, had some human value. In answer to Mr. Oldham, who spoke of gambling as a subject of conversation, he cited Peter Green's opinion, expressed in the book already quoted, that gambling was so universal a subject in factories that on days when great races took place no work was done. Leaders of industry thought work could be increased by 20 per cent. if gambling was not so much a subject of conversation.

Reverting to what Professor Eddington had termed the "immoral" aspect, the President seemed to assume that other amusements a person might go in for would necessarily cost money. That, of course, was far from being the case. Many people if not gambling would be reading in some such way as to improve their economic efficiency, possibly making money or even finding that they might be improving their minds, which were of value, even if not of money value. Mr. Katz had drawn an interesting comparison between tragedy and turning the inevitable risks of human life into gambling; a valuable comparison as far as it went. Actually the facts went a great deal further, because one did not hear of many instances in which a passion for tragedy had deprived children of their Sunday dinner, whereas in the case of a passion for gambling that often happened. Could Mr. Robson say what the last prime year was?

Mr. **Robson**: 1913.

Mr. **Hope-Jones**: Could anybody except Mr. Robson tell me the factors of 1927?

This concluded the discussion.

MATHEMATICAL NOTES.

987. [D. 2. a. α.] *The Cauchy-Maclaurin integral test and the Euler-Maclaurin summation formula.*

The integral test loses much of its significance and generality through the introduction of unnecessary sufficiency conditions. This note suggests an alternative statement and proof.

"If

(*a*) Σa_n *is a series of positive terms,*

(*b*) $f(x)$ *is such a function of* x *that*

$$a_{r-1} \geqq \int_{r-1}^{r} f(x)dx \geqq a_r \quad (r=2, 3, \dots n),$$

where the integrals involved are Riemann integrals then $\sum_1^n a_r$ *and* $\int_1^n f(x)dx$ *either converge together or diverge together.*"

From (*b*) we have

$$\sum_2^n a_{r-1} \geqq \sum_2^n \int_{r-1}^{r} f(x)dx \geqq \sum_2^n a_r,$$

or

$$S_n - a_n \geqq \int_1^n f(x)dx (= I_n) \geqq S_n - a_1.$$

From the right-hand inequality and (*a*) it follows that if I_n tends to a limit, say I, when $n \to \infty$, then so does $S_n - a_1$.

From the left-hand inequality and (*a*) it follows that if S_n tends to a limit when $n \to \infty$, then so does I_n (for $\lim a_n = 0$).

And the inequality relation between the limits is clearly

$$S \geqq I \geqq S - a_1.$$

From hypothesis (*b*) we see that the restrictions commonly imposed on $f(x)$ are not necessary; $f(x)$ need not be (i) positive, or (ii) monotonely decreasing for $x \geqq 1$, or (iii) such that $f(n) = a_n$ for every n.

The conditions (i), (ii), (iii) are of course sufficient to justify the application of the integral test and are such as hold in many practical examples. But they are not necessary. The only restriction that need be placed on $f(x)$ is that its mean value for the rth interval shall lie between a_{r-1} and a_r for every r; $f(x)$ may vary in any manner provided only that this condition is fulfilled.

Nor do we need the supposition that $f(x)$ is continuous for all $x > 1$ in proving the integral test.

Remarks similar to the above may be made on the Euler-Maclaurin summation formula, which is an expression for the difference between S_n and I_n. A simple proof of the formula is here given.

We have identically

$$\text{I.} \quad a_1 + a_2 + \dots + a_n - \int_1^n f(x)dx = \tfrac{1}{2}(a_1 + a_n) + \sum_2^n \left\{ \tfrac{1}{2}(a_{r-1} + a_r) - \int_{r-1}^{r} f(x)dx \right\}.$$

If now we assume the further condition that

(*c*) $f(x)$ *is continuous in the* r*th interval for every* r,

we have, by integration,

$$\int_{r-1}^{r} x f'(x)dx = rf(r) - (r-1)f(r-1) - \int_{r-1}^{r} f(x)dx$$

and

$$\overline{(r-1} + \tfrac{1}{2}) \int_{r-1}^{r} f'(x)dx = \overline{(r-1} + \tfrac{1}{2}) f(r) - \overline{(r-1} + \tfrac{1}{2}) f(r-1),$$

and hence, by subtraction,

$$\text{II.} \qquad \int_{r-1}^{r} (x - \overline{r-1} - \tfrac{1}{2}) f'(x)\,dx = \tfrac{1}{2}[f(r-1) + f(r)] - \int_{r-1}^{r} f(x)\,dx.$$

If we now assume that

$$(d)\ f(r) = a_r \text{ for every } r,$$

the identity I gives, if we use II,

$$f_1 + f_2 + \ldots + f_n = \int_1^n f(x)\,dx + \tfrac{1}{2}(f_1 + f_n) + \sum_2^n \int_{r-1}^{r} (x - \overline{r-1} - \tfrac{1}{2}) f'(x)\,dx.$$

Or, as it is often written,

$$f_1 + f_2 + \ldots + f_n = \int_1^n f(x)\,dx + \tfrac{1}{2}(f_1 + f_n) + \int_1^n (x - [x] - \tfrac{1}{2}) f'(x)\,dx.$$

The extra conditions (*c*) and (*d*) are sufficient to justify the use of the Euler-Maclaurin formula; $f(x)$ need not have a continuous differential coefficient over the range 0 to n.

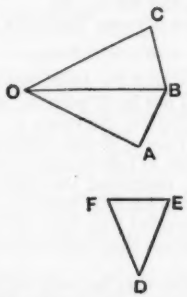
It is further interesting to note that the expression within the brackets { } in I is the difference between the Arithmetic mean of two terms and the weighted mean value of $f(x)$ taken over the corresponding interval.

V. NAYLOR.

988. [R. 1. a.] *Proof of the Formula for acceleration of a point moving with uniform speed in the circumference of a circle.*

Consider the motion of a point moving with uniform speed v in the perimeter of a regular polygon of n sides whose circumcircle has radius r.

Let O be the centre of the circle, and A, B, C three successive vertices of the polygon.



Let DE, DF represent in magnitude and direction the velocities in AB, BC respectively. Then EF represents on the same scale, in magnitude and direction, the change of velocity that takes place at B.

Hence that change is $= v \cdot \dfrac{EF}{DE} = v\dfrac{AB}{OA}$ since the triangles DEF, OAB are similar.

If t be the time taken to move from A to B, the average rate of change of velocity is $\dfrac{v}{t}\dfrac{AB}{OA} = \dfrac{v^2 AB}{vt \cdot OA} = \dfrac{v^2}{OA}$, since $vt = AB$.

Now let $n \to \infty$. In the limit the polygon becomes a circle of radius $OA = r$.

Hence the acceleration of the circle is $\dfrac{v^2}{r}$.

Also EF, being equally inclined to DE and DF is parallel to BO, *i.e.* the acceleration is towards the centre.

R. F. M.

REVIEWS.

Studies in the Theory of Numbers. By L. E. DICKSON. Pp. x + 230. 18s. 1930. (University of Chicago Press.)

Professor Dickson's name is renowned for the numerous and important services he has rendered to the Theory of Numbers. His characteristics as an author are also well known through his many books. He combines originality and discretion in his choice of material with originality in its presentation, and a thorough treatment of details often involving the mastering of lengthy and intricate memoirs, from the reading of which many a mathematician would shrink. Finally, he is able to utilise the efforts of the research school which he has developed and of which he is the stimulus.

His present book exhibits all these characteristics. It deals chiefly with results in the arithmetical theory of quadratic forms of three or four variables. A brief indication of some of its contents may be useful. The first section is concerned chiefly with the deeper properties of indefinite ternary quadratic forms and a revision of Meyer's proof of the equivalence of two such forms under comparatively simple conditions. This is then applied to Meyer's theorem on the necessary and sufficient conditions for integer solutions of the indeterminate equation

$$ax^2 + by^2 + cz^2 + dt^2 = 0.$$

The second section contains a detailed account of some theorems on the minimum values assumed by indefinite quadratic forms in two, three, four variables for integer values of the unknowns. Most of those for two, three variables are due to Markoff, while strangely enough those for four variables were completely overlooked until just found by Oppenheim of Dickson's graduate school.

The third section contains a detailed investigation of Seeber's and Dirichlet's method of defining a reduced definite ternary quadratic form so that there is only one reduced form in each class. There is also a useful table of reduced definite forms of determinant ≤ 50, a table for indefinite forms being given in the second section. The section ends with an account of the analytical methods developed recently by Hardy and myself for finding the number of representations of a given integer as a sum of n squares.

The book also contains proofs of the author's theorems on universal forms, *i.e.* those which assume all integer values.

So much then for a general survey of the substance of the book. In the preface, the author refers to his systematic exposition of both old and new results in the arithmetic of quadratic forms chiefly in three variables. He also mentions that proofs of known facts, developed in his book in detail and with minute care, have been in the past erroneously stated and inadequately proved. He states that practically no help was to be had from Bachmann's two large volumes on Quadratic Forms which contain the same errors as the original articles and which impose very restrictive conditions on the topics treated, but without explicit mention in the theorems.

These remarks ought to be supplemented. Obviously the title of the book allows the author complete freedom in his choice of material. But Dickson's preface does not make clear that there are a great number of fundamental results in the theory of ternary forms given in Bachmann and in later papers upon which he never touches.

Thus no mention is made of theorems dealing with the transformation into each other of forms of the same genus by means of rational transformations; or of the methods of finding the weight of a genus, or the class number.* Nothing is said of Selling's or Hermite's methods of reduction. Though great emphasis has been laid on indefinite ternary forms, no method is given for

* The first formulae were given without proof by Eisenstein in 1851 in *Crelle*, vol. 41. Apparently they were proved by W. A. Markoff in 1894 in the not easily accessible *Proc. Math. Soc. Univ. Khrakov.* (2) 4, unknown to Bachmann, and also to me when I found proofs in 1916 and published them in the *Messenger of Mathematics*, 47 (1918). Kořínek in 1926, 1927, unaware of the previous work, published the most general results in the *Bulletin international de l'Académie des Sciences de Bohême.*

defining a reduced indefinite form, nor is it even proved that the class number* is finite for indefinite ternary forms of given determinant. Further, I should like to have seen the theory for the ternary form presented in such a way as to show the connection with the theory of forms in n variables.

It is true that Bachmann often proves theorems under restrictive conditions. But this is a matter of policy, and a great deal is to be said for making familiar to readers important and difficult theories and results by emphasizing the salient features of the argument. Obviously it is then often necessary to take a simple case of a general theorem and reduce to a minimum troublesome details which do not introduce any new principle and which cause a great deal more difficulty to the reader than they do to an expert like Dickson. If the reader is sufficiently interested, he can consult the original memoirs, and now, for some results, Dickson's *Studies*. Many new investigations have arisen in this way, for general outlines often act as a great stimulus. Certainly, I myself am under very heavy obligations to Bachmann's two volumes as a source of inspiration.

Finally, though Bachmann may reproduce occasionally the errors of the original, even Dickson is not immune from error. Thus in his treatment of Meyer's † theorem on

$$ax^2 + by^2 + cz^2 + dt^2 = 0$$

he misquotes one of Meyer's results,‡ when $\frac{1}{2}abcd \equiv 5 \pmod 8$, and so his work is incomplete, as he does not give necessary and sufficient conditions for solvability. To err, however, is human, and I need only conclude by saying that I am glad to find a place for Dickson's book on my bookshelves not too far from Bachmann.

L. J. MORDELL.

Fünfstellige Funktionentafeln. By K. HAYASHI. Pp. viii + 176. Rm. 30. 1930. (Springer.)

This is a collection of 5-figure tables of which the first eighty pages are devoted to circular, exponential and hyperbolic functions, prepared mainly from the same author's *Sieben- und mehrstellige Tafeln der Kreis- und Hyperbelfunktionen* (Springer, 1926). We find sin, cos, tan, $\sin^{-1}$, $\cos^{-1}$, $\tan^{-1}$, e^x, e^{-x}, sinh, cosh, tanh, $\sinh^{-1}$, $\cosh^{-1}$, $\tanh^{-1}$, the gudermannian and natural logarithms at interval 0·01 up to 10·00. The tables of Bessel functions have been derived mainly from Meissel and from Watson. A table of Zonal Surface Harmonics is said to have been calculated specially. It agrees up to $P_7(x)$ with the table in Dale's *Mathematical Tables*. The tables of elliptic functions are based on those of Legendre, while values of the theta and gamma functions have been computed specially.

The present writer has already pointed out that the author's seven-figure tables are among the most inaccurate tables in existence—over 1000 gross errors having been detected to date. It would not be too strong a condemnation to say that this work is a disgrace to the art of table making. It becomes appropriate, therefore, to examine the accuracy of the tables now before us. The first four pages were checked, with the following results:

Gross errors, due to	
(*a*) corresponding errors in the 7-figure tables	9
(*b*) careless copying or proof reading	4
Errors of a unit in the last decimal, due to	
(*a*) errors in the 7-figure tables	4
(*b*) careless rounding off	9

In the next thirty-six pages the examination was confined to values known to be erroneous in the 7-figure tables; this led immediately to the discovery of 35 more gross errors, and 3 of a unit in the last decimal. These figures speak for themselves, and the conclusion is forced upon us that no confidence

* I have found a very simple proof of results of this kind, which will be published shortly.

† On March 13, 1930, some months before the appearance of Dickson, I read to the London Mathematical Society a paper containing a very simple and elementary proof of Meyer's theorem, which is appearing shortly in *Crelle's* Journal.

‡ He gives it correctly on p. 433 of vol. 2. of his *History*.

whatsoever can be placed in the accuracy of the tables. Another word of caution must be uttered, in case the reader should think that it would be safe to use the tables, with due precautions in the matter of differencing any values used. In the 7-figure tables there are "nests" of errors, in which a large number of consecutive values are affected by a constant error, which would not be detected by differencing except at the beginning or end of the run. The worst case, in which 80 values of $\log_e x$ are in error by 1 unit in the fourth decimal (of a 10-decimal table) has evidently been discovered before or during the making of the 5-figure tables.

The tables, although printed in good type, suffer from a serious defect in that too many of the leading figures have been omitted. This has not saved any space or cost, and it adds greatly to the risk of error. As is usual with books from the house of Springer, the price is very high. L. J. C.

Siebenstellige Werte der trigonometrischen Funktionen von Tausendstel zu Tausendstel des Grades. Bearbeitet im Auftrage der optischen Anstalt C. P. Goerz Akt.-Ges. von J. PETERS. Pp. 368. Rm. 18 (bd. Rm. 21). (Teubner.)

This table is not new; it was originally published in 1918 by the Goerz Optical Company, for whom it was prepared by Professor Peters of the Berlin Rechen-Institut, the doyen of table makers, but has now been taken over by the firm of Teubner. It contains 7-figure natural values of the functions sine, cosine, tangent and cotangent at interval 0°·001. The advantages of the decimal division of the degree (*not* of the quadrant) are being more generally recognised every year, especially with the growing use of calculating machines. Apart from the obvious convenience when adding and subtracting angles, there is an advantage over the sexagesimal system in that the working unit for a 7-figure table, namely 0°·00001 or 0″·036 is more appropriate than either 0″·1, which is too coarse, or 0″·01, which is too fine.

The arrangement of the tables is a little disappointing. Instead of following the usual semi-quadrantal arrangement sines are tabulated from 0°-90°, followed by tangents from 0°-90°, with co-function arguments at the foot and right of each page, so that the four functions of any angle occur on four different openings. It hardly seems likely that this arrangement was the choice of the editor; it was probably dictated by the optical company for whom the tables were prepared, as opticians do not use cosines and cotangents. The 7th decimal of the tangents has been dropped at 45°, 20° too soon, as an angle just over 45° can be determined to 0°·00003 only from its 6-decimal tangent.

The tables were prepared by interpolating to tenths the values in the *Trigonometria Britannica* of Briggs-Gellibrand (Gouda, 1633). The name of Peters is a sufficient guarantee that they are accurately computed and printed. The book is well printed on good paper, and the price is reasonable. L. J. C.

Applied Mathematics for Engineers. By T. HODGSON. Vol. I, Graphical Statics. Pp. viii + 184. 9s. 6d. Vol. II, Dynamics, with an introduction to Calculus. Pp. viii + 294. 13s. 6d. 1930. (Chapman & Hall.)

These volumes constitute another addition to that ever-growing library, the existence of which seems to be based on the assumption either that engineers require some form of mathematical training different from that of the common herd, or that the average engineering student necessarily requires special spoon-feeding in this direction.

A first perusal of these books raises two inter-connected questions. What is Applied Mathematics, and where does the teaching of Applied Mathematics as such end and that of the theoretical analysis of engineering problems begin? For example, should a book with the title *Applied Mathematics* devote some eighty pages to a quite standard treatment of the elements of the Calculus? Surely the library already mentioned has a number of volumes dealing with this under the title *Pure Mathematics*. Again, the treatment of such things as the method of sections applied to structures, of such trusses as the Finck and the Bollman, and of the effect of wind pressure on a roof truss, is unnecessarily encroaching on the domain of the engineering lecturer in structures.

The two books under review effectively illustrate the difficulties one meets directly one attempts to provide a standard course of mathematics particularly adapted for one class of students.

This difficulty is accentuated by the fact that the particular distribution of the subject and its attendant engineering subjects among the various lecturers in a training centre depends to a great extent on the available staff of that establishment. Whether the author has met these difficulties successfully is open to question. The claim is made that these two volumes should have considerable value as text-books for higher forms in schools, and Volume II is referred to as an elementary text-book on Dynamics. It is therefore fair to suppose that they are designed to appeal to the student with only slight knowledge of mechanical principles. Volume I starts with a chapter on the Theory of Vectors and then proceeds to apply this matter to the ordinary standard statical problems. This treatment while being quite logical and sound is somewhat inclined to teach the reader automatically to solve specific paper problems without the understanding of the actual mechanical principles involved.

When teaching elementary dynamics the difficulty always arises as to when one should start to introduce the use of the calculus. In Volume II, the author states in the preface that he assumes no knowledge of this subject and devotes Chapters IV and V to its elements. Yet on page 5, in a chapter devoted to the motion of a point we find the proof of $d(t^n)/dt = nt^{n-1}$. This in common with a number of other similar proofs in the early part of the book is either premature or unnecessary. The later chapters, when a knowledge of the calculus can be boldly assumed, are quite satisfactory. With the deletion of a considerable amount of matter which does not come within the scope of the title and with a certain amount of rearrangment these two volumes could be brought within the size and price of one. J. M. DICKSON.

(1) **First Lessons in Geometry.** By H. S. HALL and F. H. STEVENS. Pp. (ii + 42 + ii). 1s. 1927. (Macmillan.)

(2) **Stage A Geometry.** By R. W. M. GIBBS, B.A., B.Sc. Pp. (viii + 120). 2s. 1927. (Black.)

(3) **A New School Geometry.** Part I. By W. J. WALKER, M.A. Pp. (xiv + 160). 2s. 6d. 1927. (Mills & Boon.)

(1) This consists of Part I of the author's well-known *Shorter School Geometry*.

(2) A useful and comprehensive book for class use on the preliminary stage in teaching geometry. It is questionable whether all the instructions supplied will be needed and whether some of the attempts at realism in the plans are helpful, but the exercises are varied and suggestive.

(3) This book is written in agreement with the recommendations of the Committee of the Assistants Masters' Association on the teaching of geometry and embodies some of the ideas of the Mathematical Association Report.

The author has added some historical notes and reproduced pages of old books on Geometry. It is difficult to see that the proofs given in Stage III Theoretical Geometry of the congruence of triangles advance the position beyond that arrived at in Stage II, Fundamental truths. Most teachers of mathematics have their own methods of approach and so many introduce the idea of a locus early that they will be disappointed to find the treatment of loci postponed to the end of the book. L. ASHCROFT.

Elementary Trigonometry. By C. V. DURELL and R. M. WRIGHT. Pp. (xviii + 288 + 32 + xxiv). 5s. 1927. (Bell.)

Many teachers will be grateful for the plentiful supply of examples and problems in this book, particularly those requiring calculations from a figure. The book is so well done that it is a pity to find the statement that a new definition of the ratios is required when the general angle is reached. It may be wise to do a large amount of work with the ratios of an acute angle before tackling the general angle but surely the use of projection at once does away

with the need for a new definition at any stage. Again, any student touching the idea of circular measure must have some understanding of the idea of ratio, and the statement on page 161 and elsewhere that the number of radians in an angle is called the circular measure of an angle is misleading. The inclusion in the book of tables of logarithms, natural ratios and logarithms of the trigonometric ratios adds to its use. L. ASHCROFT.

(1) **Fundamental Arithmetic.** By P. B. BALLARD, M.A., D.LITT.

Pupil's Book VI. Pp. (iv + 96). Paper cover, 1s. 1d. Limp cloth, 1s. 3d.

Teacher's Book VI. Pp. 92. Limp cloth, 2s. 3d. 1927. (Univ. of London Press.)

(2) (i) **Junior Test Papers in Arithmetic.** (ii) **Senior Test Papers in Arithmetic.** By W. G. BORCHARDT. Pp. (50 + xii). 1s. 3d. each. 1927. (Rivingtons.)

(iii) **Junior Test Papers in Arithmetic.** By A. E. DONKIN. Pp. (iv + 84). 1s. 3d. 1927. (Pitman.)

(3) **Wide Outlook Arithmetic.** Book II. By C. W. SAURIN. Pp. 80. 1s. 1930. (Blackie.)

(4) (i) **Arithmetical Dictation.** By A. WISDOM. Books I-IV. Pp. xii, 32 each. 1s. each. (Or in one vol., 2s. 6d.) 1927. Books V-VII. Pp. viii, 150. 3s. 6d. (Or in three vols., 1s. 3d., 1s. 3d., 1s. 6d.) 1930. (Univ. of London Press.)

(ii) **Mental Arithmetical Test Papers.** Pp. 48. 6d. (Bell.)

(iii) **Rapid Arithmetic Calculations.** Parts I, VI. Pp. 40 + 8, 40 + 12. Without answers, 4d. and 6d. With answers, 6d. and 8d. (Oxford Univ. Press.)

(5) **Experimental and Practical Mensuration.** By J. T. SLATER. Books I-III. Pp. 108, 176, 278. Pupil's Books, 1s. 2d., 2s. 6d., 2s. 6d. Teacher's Book, 1s. 6d., 2s., 3s. 1922, 1922, 1927. (Oxford Univ. Press.)

(1) Dr. Ballard writes a long introduction to his fundamental arithmetic and with much of it most teachers will agree. On turning to the examples they will be surprised to see exercises in recurring decimals and probably find difficulty in deciding by what sequence of study Dr. Ballard means the student to proceed to obtain the desired joy of attainment. They will also probably violently disagree with some suggestions in the book, *e.g.* that $\sqrt{7} \times \sqrt{7} = 7$ requires *any* intermediate step.

(2) Useful collections of sums such as are set to-day, including substitution in formulae and calculations of lengths and areas from given plans.

(3) Ordinary examples in arithmetic on simple rules.

(4) The first named contains an introduction and hints on giving examples in mental arithmetic to a class which may be useful to teachers unfamiliar with method described. The questions given are helpful and suggestive and the answers are wisely placed at the side of the page. There seems a little too much method in the sets of ten questions.

The other books contain classified examples.

(5) These three books are profusely illustrated and propose a course in mensuration from the investigation of a square inch to the area of an ellipse. They would be of use to a teacher in providing hints for lessons, but few class teachers would wish such a large proportion of the lesson in print for the use of the class, and most teachers will be concerned at the hasty generalisations in the book. L. ASHCROFT.

(1) **Everyday Arithmetic and Accounts. First Year Course.** By W. S. BEARD. Pp. 64 + xvi. 1s. 6d. (or in wraps, 1s.). 1929. (Oxford Univ. Press.)

(2) **Commercial Arithmetic.** By F. L. GRANT and A. M. HILL. Pp. x + 430. 5s. 1930. (Longmans, Green.)

(3) **A Concise Arithmetic.** By C. V. DURELL and R. C. FAWDRY. Pp. xii+244 (+xxviii). 2s. 6d. (with answers, 2s. 9d.). 1930. (Bell.)

(4) **The Essentials of Arithmetic: a Manual for Teachers.** By R. WALKER. Pp. 378. 6s. (or in two Parts, 3s. 6d. and 3s.). 1929. (Harrap.)

(1) A first year's course for Central and Modern Schools which "have a commercial bias". It deals with all the elementary processes used in the home and in retail business, beginning with Compound Addition and ending with Percentage Profit and Commission. Stocks and Shares are not included.

(2) The fifth edition of a book first published in 1902. It now covers the latest requirements of the Chartered Accountants' Intermediate Examination. Compound Interest and Annuities are dealt with at considerable length. There is a useful collection of typical Examination Papers at the end.

(3) The authors describe this book as a "careful adaptation" of their larger work on the same subject. It will be used in schools where it is necessary to cover the ground as rapidly as possible, and the aim of the authors has been to insist on the quickest method from the very first. In a book like this the first sections will serve merely as an introductory revision, and for the first forty-six pages the only numbers used are integers (except in so far as simple fractions are necessary for "Practice" sums)—but even here negative numbers are introduced. The book then proceeds to Decimals and Fractions, and so to Proportion and Ratio. In Multiplication the process begins with the left-hand figure, which is placed under the right-hand figure of the multiplicand; in Division the quotient is placed above the dividend. In the actual working of these processes for Decimals the decimal point is ignored; it is inserted in the result after using a rough check. The rest of the book completes the usual School Certificate course. There is an adequate supply of special exercises in each section; besides these there are nine sets of Miscellaneous Exercises and sixty Revision Papers inserted at intervals in the book, which ends with ten half-hour papers for exercise in rapid computation. It is to be hoped that many schools will use this carefully constructed and well-printed book.

(4) This book is in two parts; the first is a text-book for student teachers, the second is a collection of examples, arranged to correspond exactly with the first. Training Colleges may find the work useful. A certain freedom is allowed to the reader in choosing his methods. Thus the three methods of simple subtraction are described, "equal additions", "decomposition" and "complementary addition". It may be interesting to note that, in the books mentioned above, Messrs. Grant and Hill insist on the third of these (the "shop" method), while Messrs. Durell and Fawdry give the other two as alternatives, with a strong recommendation of the first or traditional method. In this book for teachers multiplication begins with the left-hand figure, and the units figure of the multiplier is written under the corresponding figure of the multiplicand; this arrangement is also used when decimals are introduced. "Standard form" for multipliers and divisors is explained but not insisted on.

T. M. A. COOPER.

A System of Chemical Arithmetic. By F. C. LAY. Pp. 40. 1s. 1929. (Harrap.)

A useful collection of problems of the kind set in the Chemistry Papers of School Certificate Examinations. The problems are arranged in groups and short explanations are given.

T. M. A. COOPER.

Test Examinations in Mathematics. By A. S. PRATT. Third edition. Pp. 66. 1s. 6d. (without answers). 1929. (Methuen.)

Matriculation Advanced Mathematics Test Papers. By A. S. PRATT. Pp. 76. 1s. 9d. (with answers). 1929. (Methuen.)

Higher Certificate Mathematical Test Papers. By A. S. PRATT. Pp. vi+80. 1s. 6d. (with or without answers). 1928. (Methuen.)

This is a very useful set of books. The papers in the first cover the syllabus of a School Certificate or Matriculation Examination and are on a two-hour basis; those in the second and third are on a three-hour basis. In all three books the papers are grouped according to subjects. The questions in the third book are suitable for "Intermediate" degree examination candidates.

T. M. A. COOPER.

CORRESPONDENCE.

To the Editor of the *Mathematical Gazette*.

LOGARITHMS.

In the last two numbers of the *Mathematical Gazette* Dr. Henderson and Mr. Fletcher have traversed some of the ground so ably covered by Dr. Hutton in his *History of the Calculation of Mathematical Tables* which forms an introduction of 125 pages to his famous tables.

When I got these tables a good many years ago I was much attracted by the ingenuity of Briggs's method of calculating the logarithms to base 10 of prime numbers described by Dr. Henderson on p. 254 of this journal. Hutton, p. 63, says . . . " only a few of the first figures of the powers in the first column are retained, those being sufficient to determine the number of places in them " ; . . . " Indeed these multiplications might be performed in the same manner, retaining only the first three figures and those to the nearest unit in the third place ; which would make this a very easy way indeed of finding the logarithms of a few prime numbers."

When I read this I asked myself what becomes of the errors which, + or −, may amount to 5 in the 6th place. The processes of squaring and multiplying will cause these to grow at first slowly but later with terrible acceleration, and soon the number of figures will be wrong. Happily Hutton had committed himself to the 3 and 4 digit fallacy, and the first of these was quickly disposed of with a 50 cm. slide rule, which gives the third digit with certainty even though the first digit is 9. Barlow's tables provided the means for disposing of the 4-digit fallacy nearly as easily. It was obvious then that with 5 or even more digits only retained in the operations the errors would invade the first digit in time.

In the case of $\log_{10} 2$ the whole disturbance hangs on to the square of 1024, which is the first figure cut down to 5 digits. The true figure is 1048576, so 10486 is taken, which is 0·24 too much. This is the quantity which will increase like leaven, and it is quite immaterial if it is succeeded by negative errors. This gets the start and prevails.

I have calculated the values of the really true first *five* digits for the powers of 2 on p. 63 of Hutton's introduction. These are only carried up to $2^{1,000,000,000}$, sufficient to give the logarithm of 2 to nine places only. In nearly every case Hutton's figure for the first 5 digits of the power is exact, but is in three cases in error by −1. If the process is carried out with 5 figures only, taking always the nearest fifth digit in each square or product, the growth of error increases with increasing rapidity. Thus taking only the powers of 2, which are the 10th, 100th, 1000th, 10000th and 100000th, the errors are respectively 0, $2\frac{1}{2}$, 22, 409, and one digit too many, so at this point the process has failed.

It is certain that Briggs himself, who seemed in no way dismayed by the prospect of doing innumerable long multiplication or division sums with numbers of 20 or 30 digits, did use far more than five digits for his calculations, but in explaining the process I expect he only gave the first few digits merely as an indication, and that Hutton jumped to the conclusion that these only were needed and went further and proclaimed that four or three digits only were sufficient.

However, the matter may be looked at in a far more fundamental or general way. Suppose it is desired to find $\log_{10} 2$ to thirty places. The 100th power of 2 has 31 digits, and its logarithm = 100 times log 2. So its logarithm must be found to 28 places. The Hutton fallacy asserts that in order to do this it is immaterial whether all the digits in 2^{100} are utilised in the calculation or all are discarded except the first 5 (or 4 or 3) ! The fact is that at the beginning of the process the quantities must be known to at least the number of places desired in the logarithm, and then at each 10-fold increase in the index, when

Log 1·3712	88574	23862	35368	61362	10629	96899	58844	85848	46566	95033	93510	0
= ·13712	88574	23862	35368	61362	10629	96899	58844	85848	46566	95033	93509	9

another figure has been established and those remaining are one less in number, one digit can be dropped, until at the end of the process only 3, 4 or 5 remain. I have no access to Briggs's original papers, but I suggest as a possibility that Briggs indicated this, and that Hutton did not distinguish between the tail end of the process and the whole process. I would add that the process is no more convenient for "small" primes than for any primes.

Having thus found that Hutton's statement that three or four digits would be sufficient, or even that five would be, was wrong, I made a note in my copy to that effect, but I did not publish the matter, as a 120-year old error in an obsolete process did not seem worth raking up, but now that Dr. Henderson is using your valued columns to perpetuate this error and not one of your mathematical readers has corrected him, I think it well to put the matter right.

While on the subject of logarithms I should like, if this letter is not already too long, to refer to some correspondence in the *English Mechanic* which began in November 1917. Mr. E. M. Nelson gave the Oliver Byrne number 13712 ... to 16 digits. I was interested in this sufficiently to verify the accuracy of the statement that this number and its logarithm to base 10 had the same digits. I found only the first 13 correct, and so I showed how, knowing the gradient of the curve $y=\log_{10}x$ and of $y=x/10$ for any value of x, the difference of the gradients is known, and this is the convergence or divergence, as the case may be, between the two curves. If, then, at any point x the number and its supposed logarithm differ at all, this divergence gives the point where they agree. If the error is accurately determined, each new application of the process will about double the number of correct digits. I accordingly calculated this number to 20 places. I then noticed that the first eight digits were exactly equal to $277\times1000001\times5\div101$, all of which have their logarithms given in Hutton's table to 61 figures. So I then calculated the number to 40 figures by two methods, one using Hutton's tables and interpolation, and the other and much shorter by finding the Naperian logarithm after dividing by the number expressed by the first eight digits. This was so rapidly convergent that the 40 places were found, and then being multiplied by ·434 ..., to 40 places, the logarithm to base 10 was found. It was satisfactory to find that the logarithm and the number differed only by 1 in the 40th place. I then said that the same process repeated would with the aid of Hutton's 61-figure table give the first 61 figures. In the following March I received from Mr. W. O. Murdoch of Aberdeen the result to 61 places, again the divergence is 1 in the 61st place. These have never been published, and it is possible you may care to put them on permanent record. I quite realise that these very accurate determinations are not of any "use", and that such calculations are as futile and as fascinating as crossword puzzles or games of patience with cards, and it must be remembered that people deemed otherwise sane are guilty of these futilities.

C. V. Boys.

I am exceedingly grateful to Prof. E. H. Neville for obtaining the loan for me of Oliver Byrne's Method of Calculating Logarithms (1849). The book is very interesting in revealing Byrne as a clever arithmetician and crank at enmity with the orthodox and unsparing in his observations. He shows how he calculated his series of 'Oliver Byrne' numbers beginning with the 1·371... number, which he says he gives correctly to 16 places (p. 68). Of these the last three are wrong if my result confirmed by Mr. Murdoch is right! So apparently Mr. Nelson correctly gave his incorrect result.

ERRATA.

Vol. XV. p. 300, l. 2 up. *Delete final* d.

p. 303, l. 5. *For* one-twelfth *read* twelve.

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